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THE PSYCHOLOGICAL REVIEW

A RE-EXAMINATION OF THE CONCEPT OF INSTINCT

W. C. ALLEE
Department of Biology, University of Florida
HENRY W. NISSEN
Yale University and Yerkes Laboratories of Primate Biology, Inc.
AND MEYER F. NIMKOFF
Department of Sociology, The Florida State University

INTRODUCTION

[The idea of inviting experts or authorities in related fields to compare and evaluate their knowledge with regard to certain common concepts, is not new—although it has certainly been tried too infrequently. This plan was adopted at the annual convention of the Florida Psychological Association at Daytona Beach, Florida, in April 1952, when a zoologist, a psychologist, and a sociologist pooled their resources in re-evaluating the concept of instinct.

All were leaders in their fields. Dr. W. C. Allee, Head of the Department of Biology at the University of Florida, and formerly Professor of Zoology and Dean at the University of Chicago, presented the zoologist's position. Dr. Henry W. Nissen, Assistant Director of the Yerkes Laboratories of Primate Biology at Orange Park, Florida, and Professor of Psychology at Yale University, spoke for the psychologists. And Dr. Meyer F. Nimkoff, Head of the Department of Sociology at Florida State University, and eminent authority on the family, represented the sociologists. The symposium was under the direction and moderation of Dean Stanley E. Wimberly of the University of Florida, who asked that the speakers present their material in an evolutionary or phylogenetic order, the zoologist coming first, the psychologist second, and the sociologist third.

The contributions of the participants were so effective that the symposium became without question the high light of the two-day convention. Plans for its publication were immediately begun. What follows represents the culmination of these efforts. Since the opportunities for citing references on such a topic are almost limitless, each participant was asked not to present a bibliography.—W. N. Kellogg, President, Florida Psychological Association.]

INSTINCT FROM THE ZOOLOGIST'S STANDPOINT

W. C. Allee

A simple outline of different phases of animal behavior may help orient this discussion of instinct. One such outline recognizes a primary division of all behavior into (I) unlearned, and (II) learned behavior patterns. The former can be broken into smaller categories as follows: (*a*) poorly organized responses of animals lacking nervous systems, e.g., sponges, or early embryos; (*b*) reflex-arc reactions; (*c*) kineses, meaning a speeding or slowing of unoriented responses of whole organisms; (*d*) the oriented tropisms of sessile plants and animals and the tactic reactions of motile forms consisting, when shown in purest form, of oriented forced movements which seem to resemble reflex actions of entire organisms; (*e*) instincts,

the most complex of all phases of unlearned behavior, including many levels of behavior from variable appetitive responses to more rigid consummatory actions.

The great confusion about instincts results in part from the fact that much so-called instinctive action of man and other vertebrates, and, perhaps, of some invertebrates also is really behavior based on very early training. The training has been forgotten; the definite activity pattern remains. Sometimes the learning may be based on a single exposure to a given situation.

There are a great many definitions of instinct. These fall into two main types: (*a*) those that are primarily objective, the biologically behavioristic definitions; (*b*) the subjective ones.

Existence of the subjective emphasis is a part of the cause of justifiable skepticism regarding instincts among biologists, including, of course, psychologists. Parenthetically, the scientific part of psychology obviously is a branch of biology. The subjective definitions may be illustrated by the statement that if birds migrate south early in the fall, earlier than they are normally expected to do, this may be taken as a sign that an uncommonly cold winter is coming. The migrating birds are supposed to have a supernatural instinctive insight in the matter. Such fantasies are similar to folktales about whiteness of chicken bones foretelling the severity of the coming winter.

Good definitions of instinct are hard come by. Most of them, even of the objective, biologically behavioristic sort, are involved and difficult to interpret. There are two I wish to present. One is adapted from W. M. Wheeler, who said, approximately, that an instinct is a relatively complicated activity of an organism that is acting (*a*) as a whole rather than as a part; (*b*) as a representative of the species rather than as an individual; (*c*) without previous experience or without modification caused by experience; and (*d*) with an end or purpose of which the reacting animal has no knowledge. More recently, in 1951, N. Tinbergen, in his book entitled *The Study of Instinct*, gives his definition only after he is more than halfway through the book. Even then, the definition is offered almost apologetically in loose association with his admittedly somewhat imprecise discussion of neurophysiological relationships. His statement is (p. 112): "I will tentatively define an instinct as a hierarchically organized nervous mechanism which is susceptible to certain priming, releasing, and directing impulses, of internal as well as of external origin, and which responds to these impulses by coordinated movements that contribute to the maintenance of the individual and the species." These two objective definitions supplement each other more than they overlap. They may be tested against such activity patterns as breathing, swallowing, gland secretion, muscle contraction, all of which may be shown to occur without learning. Some types of glandular secretion, for example, can be effected by learning only with great difficulty, if at all. On casual inspection, such activities as breathing, etc. come more nearly to fitting Tinbergen's definition than they do that advanced by Wheeler, although they run aground there only on the first idea that the organism should be acting as a whole rather than as a part when displaying instinctive behavior.

Instinctive reactions represent examples of prolongation of development exhibited as behavior rather than as formation of new structure. Contrast, for example, the difference between a chrysalid and a cocoon. Each is found at pupation time in the life history of certain members of the Lepidoptera. The butterflies form a chrysalid which con-

sists essentially of a thickening of the hypodermis to form a protective covering. This thickened chrysalid is frequently attached to the walls of some crevice by a gluelike secretion. Inside its protecting covering the pupation processes take place. In contrast, the moths, as pupation time approaches, spin a more or less elaborate silk cocoon. Inside the cocoon the moth larva also develops a somewhat thickened hypodermis, which serves as an additional protection. There is no essential difference in these activities being examples of prolongation of development. One is limited primarily to the hardening of the hypodermis; the other is complicated by the presence of silk-spinning activities which represent instinctive behavior. The moth larva cannot form the cocoon until the silk glands are matured; that is, this spinning activity is also a deferred activity that cannot be expressed until the moth larva has developed functioning silk glands. Instincts and development are inextricably bound together.

Many writers speak of instinctive and intelligent actions as though these two types of behavior differ fundamentally from one another. Against this view it may be asserted that a large instinctive element enters into every form of intellectual activity; whereas instinctive actions do not usually run their courses altogether automatically and mechanically but contain in addition to fixed and unchanging components a variable element, more or less adapted to the particular situation.

An act, A, is at one and the same time normally a function of the constant, C, and a variable, V; expressed as a formula this becomes

$$A = \int (C \leftrightarrow V).$$

The constant is the instinctive biologically inherited element. The variable is the element which produces, in some cases, an appropriate modified reaction, in others, an unforeseen response to a situation. Analysis of the action into these two components is purely an abstract analysis. V and C must not be taken as two more or less opposed natural agencies pulling the organism now in this direction, now in that, as they battle for supremacy. They are merely two different aspects of the same reality. In inherited, innate, instinctive reactions C is greater than B; in intelligent action the relationship is reversed. I first read this analysis in a book by Alverdes; Yerkes gave a part of the same idea in his 1943 book on chimpanzees.

Two instances of instinctive behavior that I personally observed may be interesting, and, in addition, helpful for the discussion.

Years ago, near the Marine Biological Laboratory at Woods Hole, Massachusetts, I was trotting back from a noontime swim in a hurry for dinner when I saw a solitary wasp dragging an immobilized caterpillar across the uneven surface of the dusty road. I stopped to watch. The wasp turned before she reached the sidewalk. She passed near a low sturdy weed up which she climbed and balanced the caterpillar over a low fork some few inches from the ground. All this time the wasp and her prey had been followed by a small tachina fly, which would lay her own egg or eggs on the egg of the wasp larva. The tachina larva on hatching would feed on the larva of the wasp, while it, in turn was feeding on the paralyzed, living caterpillar.

The wasp descended from the weed, proceeded a few feet to a small burrow, which was a fraction of an inch wide and about that deep. She entered, and enlarged the burrow, digging vigorously.

After a short time the wasp left the enlarged burrow, returned along the way

she had come, but did not remember the weed-climbing incident, and did not locate the caterpillar immediately. Meantime the tachina fly had stayed near the caterpillar in place of following the wasp on her digging activities. Finally the wasp located the weed, climbed it, brought down the caterpillar and renewed her slow progress to the burrow. Now the tachina fly followed, keeping, again, about a foot to the rear.

The wasp dug a bit more, dragged the caterpillar into the burrow, and deposited her egg. Just then the tachina fly darted forward, dove into the burrow, presumably laid her own egg or eggs, emerged, and flew away. The wasp filled in the hole, leveled off the ground, and she, too, flew away. All these complicated patterns were examples of innate, unlearned behavior, both by wasp and tachina fly. There was evidence of adjustment to existing irregularities in the surface of the dust, to the size of the burrow, and, also, to the whole weed incident. However, *C* loomed much larger than *V*.

Years later I saw a more elaborate instance of essentially the same behavior —this time out in the mountains in Utah north of Great Salt Lake. There were three of us, including a poet and a psychologist, coming down the mountain trail. We disturbed a wasp carrying her numbed caterpillar enough so that she dropped her prey. We stopped, and she soon actively quartered the region, a few feet from the caterpillar. When she was beginning to waver in her quartering, I picked up the limp caterpillar and deposited it near the wasp, which soon was carrying it on to the partly excavated burrow a short distance away. She repeated the digging and burying routine. The poet sprawling with his eye close to the opening of the burrow saw the glistening egg deposited on the paralyzed caterpillar. Then the wasp filled and leveled off the burrow so that to us the spot was indistinguishable from the bare ground elsewhere except for its being darker from the moisture, which would soon dry. Still the wasp hovered near giving us the impression that she had not yet finished the operation. After a short while she settled down on a fallen cluster of three pine needles still wrapped together as they grew on the tree, transported these to the site of the burrow, and deposited them there. Then that spot looked to us as casually littered as the ground nearby, and the wasp flew away as though entirely finished.

Lorenz, Tinbergen, Baerends, and the other present-day European students of instinct study such behavior patterns by fragmenting them into smaller and smaller units, striving towards breaking them down to their reflex-arc components. At each step they attempt to discover the releasing stimuli, which they call simply releasers. In this they use passive and moving models. They report considerable and promising progress, for example, with the mating activities of grayling butterflies, stickleback fish, and various birds. The analysis of several sorts of instinctive behavior of fishes and birds has been carried fairly far in the identification of sign stimuli that serve as releasing mechanisms, finding, for example, that a bunch of red feathers is the effective releaser of territorial defense for an English robin.

Tinbergen (1951, p. 37) states that the reason why dependence of innate behavior on sign stimuli has not yet been generally recognized notably lies in the fact that so many laboratory psychologists have been studying conditioned reactions. He thinks that conditioned reactions are not usually dependent on a limited set of sign stimuli, but seem to depend on more complicated stimulus situations. The progress these men are making in an obscure phase of behavior is stimulating. It is particularly heart-

ening to see the long stagnant field of instinctive behavior, freighted with "oh my" stories such as I attempted to use in telling about the solitary wasps, beginning to yield to methods of objective analysis.

INSTINCT AS SEEN BY A PSYCHOLOGIST

Henry W. Nissen

As Dr. Allee has shown, many animals exhibit complex patterns of behavior, constant for the species and apparently unlearned, which need a name to set them off descriptively from other behaviors. In the first part of my discussion I shall try to relate the term "instinct," as designation for these uniformities, to certain other concepts, such as reflex, habit, and drive. These considerations will lead to the conclusion that "instinct" has real but limited usefulness, and to the suggestion that all behaviors may be ordered on a continuum of possible associations which are more and less readily learned.

My second point will be a criticism of the current tendency to oversimplify the problem of motivation by organizing all behavior under half a dozen biogenic drives or instincts plus a few psychogenic or secondary drives. This part of the discussion will be illustrated by recent observations of the development of sex behavior in chimpanzees.

Terms referring to the dynamic or energizing aspect of behavior must be kept sharply separate from those pertaining to the form or pattern of the behavior itself. Confusion between the two has been responsible for much of the misunderstanding and controversy about instinct and persists even today: Tinbergen, 1951, presents a diagram of "the hierarchical organization of instincts" which looks like the flow chart of a military chain of command. The top brass—that is, the top instinct—decides the strategy, lower centers direct the tactics, and the lowest-level instincts do the work. The "instinct of pugnacity" refers both to the motivation to fight, and to the manner or pattern of fighting. The term is useful only in the latter sense, which is the way I shall use it here.

The dynamic factor in behavior has been given various names, the most common one, perhaps, being "drive." Morgan has termed it "the central motive state," or CMS, and I have called it the "sensitizing component of behavior determination." The drive or sensitizing factor elicits behavior which is often such as to increase or decrease the amount of a certain kind of stimulation. Thus the earthworm may orient away from the source of light, whereas the Euglena may swim towards the light. Instead of deriving from the external environment, the stimulation may arise internally: the organism sneezes, defecates, or urinates. In other cases, the effective condition is a surplus or deficiency of chemicals in the bloodstream, presumably sensitizing certain parts of the nervous system. In still other cases, the mechanism of the energizing factor is completely unknown. You will note that I am postulating a sensitizing factor in all behavior, whether it is of external or internal origin, and whether it leads to behavior as simple as a reflex or as complicated as nest-building.

Now the behavior which ensues when a sensitizing factor is active may differ in various ways. Most obviously, it may be all over in a moment, or it may last a long time. When the goal or situation which brings relief is immediately present, the response is prompt and brief; we call it an unconditioned or conditioned reflex, tropism or taxis, or automatized habit. When the goal object or situation is not at hand, a more or less prolonged series of acts occurs. Duration in time is therefore one differentiating criterion. Further, a reflex typically is elicited by a specific, local-

ized stimulus, whereas an instinct is usually determined by a combination of internal factors plus a *class* of external stimulus patterns which, in their details, may vary considerably. On the response side, a reflex implies contraction of a particular muscle or muscle group, whereas instinctive behavior allows considerable leeway in the effector mechanisms by which a common effect is achieved.

In the absence of the goal or drive-reducing situation, the organism is forced to an indirect, extended series of acts. These longer sequences may be classified into five categories, in accordance with information available to us regarding *how* the form of that behavior was determined: (*a*) When little or no direction or selectivity is in evidence, we speak of "random" or "spontaneous" activity. (*b*) When innately determined components are conspicuous, we call the sequence an "instinct." (*c*) When learning has determined the pattern, we call it a "complex habit." (*d*) When there is evidence of reasoning and foresight, we speak of "purposive striving" or "goal-directed" activity. (*e*) When we want to remain neutral about the role of the innate, experiential, and central factors involved, we call it simply "drive behavior."

Together with the other terms designating prolonged behavior sequences, "instinct" implies the sensitizing effect of neural stimulation. An animal behaves differently when hungry than when thirsty. The drive or motivating factor sensitizes the animal to some stimuli and makes it obtuse to others. It both filters and intensifies. This selective action does *not* differentiate among the five classes of long sequences. Nor does the consummatory response, which usually gives the behavior its name, and which may be the same for random, instinctive, rational, and habitual responses. It is in the etiology of the behavior preceding the consummatory response, in the past history of the animal, that the differentiating criteria are to be sought. This demands experimental analysis. Sometimes the data are unambiguous, as when the isolated bird sings the song which is characteristic of its species. Both birds and chimpanzees build tree nests; in the former the behavior pattern is often innate, but in the apes it is evidently learned, being transmitted from one generation to the next. Often the answer is complicated, because the behavior sequence contains both innately determined and learned components. Instinct and intelligence are *not* mutually exclusive. Some of the fastest and most efficient learning that we know of is intimately related to instinctive behavior: the foraging insect must learn and remember, on the basis of a single flight, the direction or the landmarks which guide it back home. According to Baerends, the digger wasp even remembers from day to day how well each of its eight or ten burrows is stocked with provisions.

Finally, I should like to suggest that reflexes, instincts, and the inherited capacity to learn may be distributed on a continuum. What is inherited may be *a more or less specific readiness to learn.* The concept of learning implies that such readiness is rather nonspecific. But often, as in the case of the wasp, there is a readiness to learn very specific things. When such selective readiness is common to the species, the resulting behavior can hardly be distinguished from instinct or reflex.

We come, now, to a criticism of the tendency to ascribe all or most behavior to a few drives or instincts. During the past two years I have been observing the behavior of male-female pairings of late-adolescent chimpanzees. As youngsters these animals lived together, but well before puberty they were separated by sexes. Starting at least a year after the first menstruation, and at a higher

chronological age for the males, these animals were put together for periods of observation at times when the female was in swelling, that is, during the periods of physiological receptivity. All possible pairings of five sexually naive males and a like number of females were made. The observations total over 100 in number. If I had used experienced, rather than inexperienced, chimpanzees under like conditions, a conservative estimate of the number of copulations which would have occurred is 200. In my observations there were no copulations.

However, except for what Carpenter has delicately called "primary sex activity," the chimpanzees were very active during these hours of observation. These primates are notoriously inventive and varied in their behavior, and practically everything that a caged ape *can* do these animals *did* do in the course of my observations. The behaviors which occurred were both individual and social. The former included self-grooming, solo gymnastics such as somersaulting and doing cartwheels, sucking a thumb, or just sitting and thinking. Among the social interactions there were a few instances of serious aggression, many occurrences of bluffing or exhibitionistic behavior, a great deal of play-fighting, wrestling, playful slapping or boxing, tag or follow-the-leader, and mutual grooming. The point to be stressed is that, although there was no copulation, there was a great deal of social behavior, including most of the constituent acts which enter into the mating pattern.

The question now arises whether the observed behavior which did occur is properly and appropriately designated as sex behavior, as expression of the sex drive or instinct. The only excuse for doing so is *our* knowledge (*a*) that with other, experienced, male-female pairings the sex act would occur, and (*b*) that, with continuation of the prevailing conditions for a long enough time, the sex act most probably *will* occur, eventually, in each of these 25 pairings. But this is not adequate justification for ascribing these long sequences of variable behavior to the "sex drive." There was no consummatory act and no drive reduction; the reactions were not "preparatory" (except in a farfetched teleological sense) for mating.

What, then, did motivate the great variety of behavior which was observed? There are several theoretical possibilities. The old concept of youthful play as being the incomplete, nonserious expression of later biologically significant behavior patterns is particularly unsatisfactory here because, structurally and physiologically, these animals *are* ready for actual mating and its consequences. A second possibility is to postulate an independent drive, such as activity, exploration, play, or curiosity. The conceptualizations are too glib and facile—they "explain" too much too easily, and give no "handle" for experimental testing. Still another possibility is the idea of displacement reactions, as proposed by the German neonaturalists. They suggest that when the environment does not provide the stimuli or objects necessary for the development of the currently dominant instinct, reactions belonging to some other instinct will occur. This also is an *ad hoc* explanation which can be brought in as necessary to "explain" almost anything.

Instead of starting with a dozen or so drives, instincts, or propensities, under one or another of which all behavior is ordered, we may, instead, postulate a multiplicity of self-motivated activities. What were conceived of as part-activities, all contributing towards some one definite end, are thought of, instead, as a series of independently motivated acts. Every postural adjustment, every approach to or avoidance of a given ob-

ject, each episode of grooming, has its own, intrinsic motivation. To explain the vast number of movements and acts, extending over hours, and sometimes over weeks and months, as all being determined and guided by one drive, whose direct and identifiable expression can be seen only in a brief consummatory act, is pure anthropomorphism. The acrobatics, wrestling, and grooming seen in my male-female pairs of chimpanzees cover a longer period of time, and involve a greater variety of precise coordinations, than does the act of mating which takes less than a minute. But mating is all that may be strictly designated as sex behavior.

Mutual grooming by chimpanzees contains elements which, in human behavior, we call foreplay or petting and which we ascribe to the sex drive. Could it be suggested that petting is, sometimes, an end in itself, with no sinister motive towards a further ulterior goal? In chimpanzees, grooming occurs as frequently in preadolescent animals, who have had no sex experience, as it does in adults; it occurs in female-female pairs as much as in male-female pairs; and it occurs more often after mating than before. Looked at from the outside and as a whole, the goal-directedness of the behavior sequence is obvious, but that is the view seen by the human mind, not the view of the organism doing the behaving. To suppose that it is, is to endow the animal, anthropomorphically, with the foresight which characterizes the deliberate, devious, and planful behavior of man.

The behaviors legitimately and descriptively named sex, hunger, thirst, and so on, are relatively infrequent, isolated events in the flow of behavior; their motivation demands explanation no more, and no less, than do the many activities appearing in each of various sequences. To say that a given act is sometimes motivated by sex, another time by hunger, is to slur over the basic question of motivation. Differential sensitization, determining the probability of occurrence of various reactions, needs explanation. But more fundamental than the problem of frequency of elicitation, is *why* grooming and wrestling and play-biting occur at all. Since they appear when there is no copulatory drive, and more often after reduction of the sex drive than before, they must be independently motivated.

The theme of the view which I am advocating might be summarized in the words of a once-popular song, whose title is, "Every little movement has a meaning all its own." I should perhaps restate this to read, "Every little action has a motive all its own." My point, of course, is not that sex has been overrated, but rather that it already has enough to account for, without our burdening it with more than its fair share of responsibility.

A SOCIOLOGIST'S VIEW OF INSTINCT

Meyer F. Nimkoff

Since Dr. Allee, a zoologist, has dealt mainly with the lower animals, and Dr. Nissen, a psychologist, with chimpanzees, my function is to consider the question of instinct in relation to man. Although sociologists concern themselves primarily with man, they need for proper perspective on this question of instinct some knowledge of the motivations of other animals. This knowledge comes largely from zoology and psychology. Sociologists may, of course, properly study the social life of animals other than man, although few do so.

To avoid semantic difficulties, it is desirable at the outset to clarify the concept of instinct, which the two preceding discussants have done. Central to the concept are the ideas that the behavior is complex, common to the species, and unlearned. An example is the

building of nests by birds. A bird that has never seen any other bird build a nest will still build a nest if the conditions are right. There is, in fact, an inner compulsion that forces certain animals through the motions of nest-building even when the necessary materials are lacking. The nests of different species of birds may vary much more than do the nests of any one species.

Does man have any instincts? The early psychologists, William James, William McDougall, and E. L. Thorndike, drew up long lists of instincts. James said man had more instincts than any other animal. His list included sucking, crying, locomotion, curiosity, shyness, cleanliness, pugnacity, fear of dark places, acquisitiveness, love, jealousy.

About 1924 the reaction set in with the publication of *Instinct* by L. L. Bernard, a sociologist, who showed how confused was the use of the term. Covering some 400 authors, he disclosed that about six thousand urges had been called instinctive. These were of two kinds, those of a general nature like sex and social behavior, and those that were more specific like "an instinct for the piano" and "an instinct to avoid eating the apples of one's own orchard." From our perspective today, Bernard's analysis seems like a caricature. Instinct in human psychology was buried, and Knight Dunlap and John B. Watson added their nails to the coffin with their accent on conditioning and learning.

The tendency in recent years has been to reserve the term instinct to the lower animals. Man's nervous system is not as imperious in dictating behavior because it is more complex than that of other animals and is less fully coordinated at birth. For instance, it takes a human being longer to learn to walk than it does an ape. This complexity and plasticity of the human organism makes possible more and richer learning. So, in the case of human beings, we drop the term instincts and speak of organic drives, which are more general. There are also drives of a purely social origin which have been termed wishes or motives. Most of man's behavior is learned, whereas most of a bird's behavior is not learned.

The problem of instinct appears to be a matter of differences in the degree of the learning capacity of organisms. Some students object to the use of the term instinct even in reference to the lower animals because they, too, may be capable of learning and are not mechanistically repetitive in their behavior. Even the lowly amoeba can, apparently, learn to alter its behavior in response to new stimuli. Birds improve the quality of their nests with practice, and learn from other birds. So the differences between man and the lower animals can be exaggerated. The anthropomorphic danger is ever present, and man must beware of his bias in favor of himself. The evolutionary viewpoint is a wholesome corrective, emphasizing as it does differences in degree as well as differences in kind.

We can stress the fact that differences in learning ability fall along a continuum, but this need not cause us to lose sight of the fact that differences in degree may be very considerable indeed. The ants have complex behavior patterns based on the division of labor, differentiation of status, and different classes of workers. These patterns are inherent in the structure of each ant and show little change except as the ant may change. Professor W. M. Wheeler examined ants embedded in Baltic amber fifty to seventy-five million years ago and concluded that these ants had developed all their various castes just as they exist today. The larvae and the pupae were the same. They kept guest beetles in their nests and had parasitic mites attached to their legs in the same special positions as do our

species today. Apparently the ants have learned very little in fifty million years. On the other hand, man has learned much. A community like New York City with its eight million inhabitants and complex social organization is very different indeed from an Eskimo band of 10 to 100 individuals or thereabouts, a change which represents a cultural span of about 15,000 years. So we may say that while other animals may have some culture, the amount is negligible, and man is the only animal with a substantial and significant culture. The differences in cultural level among animals reflect differences in learning capacity, a biological phenomenon. Among large groups of men, like races and nations, differences in learning are the result of differences in opportunities for building culture, not differences in inherited learning ability.

Sociology depends on biology and psychology for knowledge of the inherited biological nature of man. What can sociology itself contribute to our understanding of this problem? Sociology is interested in the structure and function of social systems: that is, the total cultural organization that constitutes man's social environment. We cannot control the human environment experimentally, like that of birds or chimpanzees, to observe how individuals behave when reared in isolation. The folkways and mores, which are so important to human learning, nowhere permit this.

While it does not seem possible to hold the human environment constant by isolating individuals and denying them the opportunity of learning from the group, we can in a sense reverse the situation and observe what happens when the genetic factor is held constant, as it is when very large groups of human beings are involved. Individual differences in genetic traits are minimized or cancelled out under these conditions.

What we find in different human societies is a considerable diversity of customs. Some cultures are strictly monogamous, others polyandrous, still others polygamous. Some societies do not permit divorce, most societies frown on it but permit it, and a few encourage it. The folkways are marked by variety. This is thought to demonstrate man's highly flexible inherited tendencies.

Underlying the diversities, however, are certain uniformities. Whether it be monogamous, polyandrous, or polygamous, the family is everywhere to be found. So it is natural to ask: do the uniformities in culture the world over reflect certain common human drives that are genetically determined? Are the cultural universals an expression of man's unique biological nature?

In the case of the family, the answer seems clearly to be in the affirmative. The universality of the human family as a social institution rests on the fact that there are two sexes endowed with a sex drive and that the human infant is highly dependent on others for survival. The family is omnipresent, but the organization of the family varies greatly. So we conclude that the presence and broad outlines of this institution are determined by man's biology, but not the details of structure and function.

Cultural universals are not the only indicators of man's innate biological nature. Cultural preferences may be indicators, too. While culture patterns vary widely, some patterns are more common than others. Polygamy, the marriage of one male and several females, is much more frequent than polyandry, the mating of several males and one female. Does this suggest that polygamy is more compatible with the biological nature of man than polyandry? The evidence from the primates supports this interpretation. The male, generally larger than the female, often shows signs of jealousy and possessive-

ness. The organization of a baboon family is described by Zuckerman as consisting of a male overlord, his female or females, together with their young, and sometimes including one or more "bachelors" or unmated males.

It is not safe, however, to infer that behavior is genetically determined simply because it is found to be widespread, or even universal. Take, for example, the taboo against incest. Every society enjoins incest, although the definition varies, as does that of the family. Violation of the incest taboo is severely punished, and few if any offenses are regarded as more serious. Is the horror of incest "instinctive," as some have argued? If so, it would hardly seem necessary to have a taboo against it. The explanation of the taboo is not clear, as may be gathered by an examination of the voluminous literature on the subject. A more plausible explanation for the incest taboo is that it is the result of universal social factors and that it helps to promote social organization. It is difficult, for instance, to see how authority patterns could be maintained in the family without it. Therefore we are unable to conclude that human behavior which is universal must, by virtue of this fact, be biologically motivated. There may be universal considerations in the acquired social situation which prompt the behavior.

Moreover, behavior which is biologically induced may be modified by the culture so that its usual or normal expressions are not evident. The sex drive, for instance, can be repressed. People can be persuaded to fast and even starve to death, although it is difficult to see how the latter behavior could be universalized if society is to persist.

In short, there does not seem to be any easy formula which we can use for determining what is learned and what is genetically motivated in human behavior.

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ON THE PROBLEM OF PERCEPTUAL DEFENSE

LEO POSTMAN

University of California

In a recent issue of this journal, Howie (9) published a spirited attack on the concept of perceptual defense used by Bruner and Postman in the analysis of motivational factors in perception (2, 3, 4, 20). Since the writer had himself come to the conclusion that the facts which gave rise to this concept can be better subsumed under other, more general, principles of perception (16, 18, 21), he would have been ready to welcome Howie's statement. It appears, however, that Howie was right for the wrong reasons and used questionable polemics to make his points. The purpose of the present note is to reconsider the status of perceptual defense and to clear up some of the misconceptions which appear in Howie's argument.

Origins and Status of the Concept

Original statement of the concept. Let us begin by briefly sketching the development and present status of the perceptual defense concept. The principle was first formulated in connection with the analysis of recognition thresholds for words from different meaning classes. It was found that negatively valued stimuli had, under some conditions, higher thresholds than positively valued or neutral ones. In addition, the prerecognition responses to the negative stimuli were such as to suggest avoidance of their recognition (20). These findings led to the speculation that the recognition of negatively valued stimuli is delayed by "perceptual defense mechanisms." It was acknowledged and, indeed, emphasized from the very beginning that *perceptual defense was not in itself an explanatory principle* and that the mechanisms mediating it remained to be discovered. To quote from the first statement concerning perceptual defense: "One may inquire at this point, 'How does the subject know that a word should be avoided?' In order to repress, he must first recognize it for what it is. We have no answer to propose. . . . Of only one thing we can be fairly sure: reactions do occur without conscious awareness of what one is reacting to. Psychological defense in perception is but one instance of such 'unconscious' reaction" (20, p. 152). Clearly, then, perceptual defense was a descriptive principle of perceptual functioning—a statement about an outcome of threshold measurement—whose specific mediating mechanisms were a problem for the future.

The problem of mediating mechanisms. The next step was to speculate about possible mechanisms (4). Hypotheses about mediating mechanisms had, of course, to come to grips with the apparent paradox of perceptual defense—that the stimulus has to be somehow discriminated in order for the processes delaying its recognition to be brought into play. Upon closer inspection the paradox turned out to be a semantic problem. There may be more than one kind of discrimination, in the sense that more than one class of responses may be systematically related to variations in the stimulus. Such different discriminations may (*a*) have different thresholds and (*b*) facilitate *or* inhibit each other. It has been shown, for example, that reduced stimulation insufficient for correct verbal report (such as a brief tachistoscopic exposure) may be adequate to trip off conditioned

autonomic responses (12, 15).[1] Such autonomic responses (discrimination without awareness) may in turn delay the appearance of the correct verbal response defining discrimination with awareness. Exactly this sequence of events was proposed by McGinnies (15) in explanation of what appeared to be defensively high thresholds for the recognition of taboo words. I am not here arguing in favor of that specific hypothesis; in fact, as I shall try to show presently, other conceptualizations of these data are more satisfactory. What I wish to insist on at this point is (*a*) that perceptual defense referred to an observed property of certain recognition thresholds, (*b*) that there was nothing inherently illogical about such a concept, and (*c*) that it was possible to hypothesize specific mechanisms mediating it.

Reanalysis and reduction to general principles. The concept of perceptual defense did, however, prove an uneconomical one, or at least an expendable one. What had appeared as a special principle could, upon reanalysis, be considered a special case of more general principles of perceptual functioning. There were two sets of considerations which led to this conclusion: problems of experimental control, and theoretical parsimony.

1. Experimental control. Empirically, perceptual defense refers to the *difference* between recognition thresholds for neutral and anxiety-arousing stimuli. Clearly, perceptual defense can be inferred on the basis of such data only if other more general determinants of the observed differences in thresholds have been adequately controlled. Such control has proved difficult. For example, the striking differences between recognition thresholds for neutral and taboo words reported by McGinnies (15) can upon reanalysis be ascribed largely to differences in familiarity between the two classes of words and the effects of selective-reporting sets (7, 18). In other studies in which complex stimulus materials such as sentences (23) and pictures (5) were used, the evaluation of the relative familiarity and structural difficulty of the neutral and critical stimuli is so difficult that the threshold differences cannot be interpreted as evidence for a defensive process. Similar difficulties of interpretation arise in studies which correlate sensitivity to critical stimuli with independent personality measures of the *Ss* (5, 6, 11). Even though the personality measures purport to classify *Ss* in terms of their proneness to defensive reactions, it does not follow that the threshold differences observed do, in fact, represent different degrees of perceptual defense. A rigorous operational definition of *defense* would have to anchor the concept in *antecedent* as well as in consequent conditions. Since the antecedent conditions have not been fully specified or brought under experimental control, alternative interpretations of the observed threshold differences in terms of such variables as familiarity and selective set remain possible. Such alternative interpretations are indirectly supported by the fact that variables like familiarity and selective set *can* be experimentally manipulated and do produce variations in thresholds analogous to those ascribed to perceptual defense (18).

2. Theoretical parsimony. In the development of a general theory of perception one of the major requirements is the formulation of constructs which have the highest possible degree of generality. The applicability of such concepts should, for example, not be restricted to

[1] Before such autonomic responses can be confidently regarded as evidence for systematic discrimination rather than as artifacts of the method of measurement (cf. 7), they should be validated by means of other classes of responses (cf. 17).

special types of motivational situations. Tentative attempts at the formulation of such a theory, centering about the concept of *perceptual hypothesis*, have been published elsewhere (1, 16). A hypothesis was defined as a predisposition of the perceiver to organize stimulus cues in specific ways. Such hypotheses are anchored on the antecedent side in conditions of stimulus input and specified conditions of the organism (including drives and motives) and on the consequent side in systematic perceptual responses (discriminations, verbal reports, etc.). Hypotheses vary in strength, i.e., they vary in the amount of stimulation necessary to arouse, confirm, or deny them. We have found the concept of hypothesis useful in the analysis of a considerable body of experimental data, including the phenomena of perceptual defense which can now be economically conceptualized in terms of interference among competing hypotheses. "What appears to be perceptual defense results from the dominance of strong alternative hypotheses rather than from active repression of the inimical or dangerous. In the presence of partial information, strong hypotheses incompatible with the threatening stimulus may be evoked. . . . If this is the case, the subject will appear to be defending himself against perception. . . . If, however, hypotheses related to the negative stimuli are strong, the opposite of defense will appear to operate" (16, pp. 256 f.).[2] Thus, the demands of theoretical parsimony led us to abandon perceptual defense as a special principle of perception and to fit the phenomena to which it referred into a broader theoretical context.

Such, as we see it, is the "case history" of the concept of perceptual defense to date. The concept was suggested by challenging experimental data and served the purpose of raising useful empirical questions and sharpening theoretical issues. *The reasons for which such a concept is accepted or rejected* appear to us to be of critical importance, for they reflect the intellectual climate in which theory construction proceeds, the rules by which we agree to abide in our theorizing even though specific constructs may come and go. I believe that Howie's critique of perceptual defense is largely based on criteria whose acceptance would be dangerous to sound psychological theorizing and to experimental initiative. Thus, even though I do not now hold a special brief for the concept of perceptual defense, I feel it necessary to register my disagreement with most of his arguments.

EVALUATION OF HOWIE'S CRITIQUE

Several lines of criticism are advanced by Howie in his attack on perceptual defense—operational, logical, and, most important and pervasive of all, metaphysical. Let us now consider them in detail.

Relation between defense and other motivational principles. The principle of perceptual *defense* asserted that the perceptual process serves to minimize recognition of objects and events inimical to need and expectations. This principle was supplemented by the apparently antithetical principles of perceptual *vigilance* and perceptual *resonance* (2, 20). Vigilance referred to the empirical observation that under some conditions the organism is more sensitive to threatening or inimical stimuli than to neutral ones. Resonance referred to *S*'s tendency to give prerecognition hypotheses (i.e., to interpret reduced, un-

[2] This shift in interpretation from an inhibitory mechanism to a principle of competition among incompatible responses parallels developments in the theory of retroactive inhibition (cf. 14). It is to be noted that in his paper Howie makes no reference to the discussion quoted here or to a related statement (21) bearing on the interpretation of perceptual defense.

clear stimuli) in accordance with his needs and expectancies. As a result the probability of an early correct recognition response is increased if the stimulus actually belongs to the class of objects (e.g., words) which are the major sources of prerecognition hypotheses. The interrelation among these three principles provides Howie with his first target of attack. The criticism centers around two points: (*a*) the difficulty of operational delimitation of these antithetical concepts and (*b*) the imputed motivation behind their postulation.

Let us turn to the operational criticism first. The legitimate question is raised as to where one principle begins and the other ends, i.e., how these different concepts are to be concretely applied to empirical data (cf. also 13). It is true that some of the early statements of the three principles made their precise demarcation difficult when they were used in the speculative interpretation of observed perceptual responses. They can, however, be given adequate and mutually exclusive definitions. Let us remember that the concepts were introduced to account for observed differences in thresholds for stimuli from different meaning classes. Using the thresholds for standard "neutral" stimuli as points of reference, thresholds for stimuli from specific meaning classes which are significantly higher would define defense, thresholds which are significantly lower would define vigilance. These specific meaning classes would, of course, have to be defined *independently of the threshold data* on the basis of a theoretical analysis of the conditions of defense and/or vigilance. Ideally, the critical meaning characteristics should be produced by means of experimental manipulations of the organism, such as the establishment of a conditioned fear response (cf. 12). The critical classes of stimuli would also have to be equated to the standard stimuli in terms of other characteristics (familiarity, structural properties) relevant to their discriminability. Short of such control of antecedent conditions, alternative explanations of the threshold differences are possible and a precise demarcation of significantly "vigilant" and significantly "defensive" thresholds must remain difficult.

The situation is, of course, *in no way different* for the concepts of facilitation and inhibition in learning which Howie contrasts favorably with the defense-vigilance antinomy. When the learning of a list A has a significant influence on the learning or retention of list B, it is often impossible to tell to what extent facilitation and inhibition both entered into the transfer effect. Facilitation and inhibition must be defined in terms of the net transfer effects obtained under standard experimental conditions.

As for resonance, it is capable of direct identification and measurement in terms of the latency and frequency of different types of prerecognition hypotheses given by *S* prior to correct recognition of the stimulus. The less the latency and/or the higher the frequency of prerecognition hypotheses from the same meaning class as the stimulus, the higher is the degree of resonance. The same feature of perceptual responses has been analyzed by Solomon and Howes (8, 24) under the heading of response probability.

In summary, then, descriptive definitions allowing for the demarcation of defense, vigilance, and resonance are possible. As was pointed out above, however, theoretical parsimony leads to the abandonment of these as principles in their own right since they are reducible to special cases of more general principles.

Howie directs his major criticism at what he believes to be the reasons for the introduction of antithetical principles such as defense and vigilance. In

discussing the principle of vigilance he writes, "In this notion we have an attempt to incorporate within the theory of perception a reality principle without which resonance and defense would make veridical perception impossible and, confining the person within his economy of need determinations, would deny him any effective adjustment to conditions other than his wishes or his fears" (9, p. 309). And in another place he avers that vigilance is "brought in to meet the ultimate solipsistic difficulty of resonance and defense-determined perception" (9, p. 310). These reasons are Howie's; they were not ours. The introduction of these principles stemmed from empirical observations, not from a struggle with problems of epistemology and metaphysics. Perceptual phenomena are what they are, no matter what may be the nature of the "real," concern with which we are content to leave to the philosophers. However, if we may allow ourselves for the moment to engage in epistemological argument, it can be easily shown that a principle of perceptual defense does not involve us in the "ultimate solipsistic difficulty" from which we must then extricate ourselves by means of a principle of vigilance. Perceptual defense refers to observed *quantitative variations* in recognition thresholds, i.e., it was asserted that within the constraints of the stimulus situation the individual will avoid or delay recognition of inimical stimuli. It has never been asserted that the individual is incapable of recognizing inimical stimuli and hence is in danger of being encapsulated in an autistic world of his own; the only claim was that the thresholds for some classes of objects under some conditions of the organism were higher than for other classes of objects. By the same token, the principle of vigilance denoted the fact that under some conditions of the organism thresholds for some classes of objects are lowered.[3] As a statement concerning variations in sensitivity, perceptual defense involves neither greater nor lesser epistemological hazards than do such time-honored psychological stand-bys as "selective set" and "attention."

It is interesting to note that Howie is perfectly willing, as are we, to speak of the determination of perceptual recognition by "conditions of relative set-dominance." But then, if we are to fall in with his epistemological worries, is there not serious danger that, to paraphrase the statement previously quoted, the individual will be "confined within the economy of his set-dominances which would deny him any effective adjustment to conditions other than those fitting his set-dominances"? Selective sensitivity is still selective sensitivity by any other phrase and is, moreover, an empirical fact, whether or not it fits any epistemological preconceptions. It appears that the objection cannot be to the hypothesis of perceptual selectivity but rather to the word, "defense." And an ill-chosen word it was if unwarranted surplus meanings and overinterpretations of the term are as compelling as they seem to be.

The logical paradox of perceptual defense. Howie's second major criticism concerns the logical paradox implied by the concept of perceptual defense: "To speak of perceptual defense . . . is to speak of the perceptual process as somehow being both a process of knowing and a process of avoiding knowing" (9, p. 310). We have already considered this problem and have tried to show that it is a semantic one arising out of ambiguities in the definition of the term *knowing*. A few additional comments are in order here. It seems quite inap-

[3] The statement that certain classes of discriminations will be "minimized" or "maximized" (4), to which Howie objects, is to be interpreted in this sense only.

propriate to bar the use of a concept by constructing verbal contradictions which somehow seem to be suggested by the concept. The only contradictions which provide serious grounds for objection are incompatible operations or incompatible deductions entailed by the use of a given concept. The context of empirical investigation in which perceptual defense was used made it clear from the very beginning that it referred to a lack of one-to-one correspondence between two different sets of perceptual discriminations—those measured by correct verbal response and those inferred from other systematic response tendencies in the presence of the stimulus. What is gained by then labeling both sets of discrimination as *knowing*, and announcing that the concept entails a simultaneous process of knowing and avoiding to know? [4]

Finally, it should be noted that Howie takes cognizance, in passing, of the resolution of the paradox (9, p. 310, footnote 2) but then dismisses it and proceeds with his criticism since this resolution "involves a shift in status for perceptual defense. Instead of a basic principle of perception, it has become something to be explained away." The shift in status is of Howie's own construction. As we have pointed out repeatedly, perceptual defense was not and could not be considered an ultimate irreducible principle which did not require further analysis in terms of mediating mechanisms.

The motivation behind motivational concepts in perception. Having pointed to the operational, logical, and epistemological flaws in the concept of perceptual defense, Howie addresses himself to the motivation behind the acceptance of this concept. "Why," he asks, "should it show such vitality, why should it prove so insidiously attractive?" (9, p. 311). To analyze the intellectual motives of one's colleagues is a hazardous venture indeed, but Howie believes that there are two main sources of confusion which account for the insidious seductive power of the concept of perceptual defense: "(1) the interpretation of processes in terms of agency concepts and (2) the internalism of an exclusive emphasis on impulse" (9, p. 311).

I am still not too clear about the first of these sources of confusion since it is stated in only the most general, and nebulous, terms. To speak of perceptual defense is said to "commit a category mistake in representing 'the facts of mental life as if they belonged to one logical type or category . . . when they actually belong to another' " (9, p. 311). As far as I can gather, the main objection is to the ascription of defense to the perceptual process rather than to the person as a whole. Persons may perhaps defend themselves against perceptions, e.g., by shutting their eyes, but perceptual processes cannot. If I read the argument correctly, I submit that we are dealing with a semantic strawman which was erected only to be annihilated. When we ascribe properties to a process and then in turn attempt to explain certain observed outcomes in terms of these properties, we do not thereby endow the process with the status of an agent with aims of its own, functioning independently of the total organism.

[4] The inappropriateness of such play on words may be further illustrated by an example from learning theory. One of the fundamental assumptions of Hull's theory is that the continued reinforcement of a stimulus-response connection produces not only increments in habit strength but is also conducive to conditioned inhibition, i.e., the acquisition of a habit not to respond. Hull carefully distinguishes the conditions and quantitative properties of these two simultaneous processes and the hypothesis has been experimentally fruitful. Yet a superficial semantic argument of the type discussed here would give short shrift to Hull's hypothesis by asserting that reinforcement cannot at the same time be a condition of learning to respond and learning not to respond.

In the interests of analysis, we identify certain processes, such as perception, and try to give as full a description of their rules of functioning as possible. If we were to take Howie's injunction seriously, it would be equally inappropriate to analyze the memory process in terms such as retroactive and proactive inhibition, for after all, it is the whole man, and not the memory process, which does the remembering and forgetting! As long as the definition of processes remains anchored in antecedent and consequent conditions, we need not aggravate our problems with worries about their status as independent or dependent agents.

The second alleged source of confusion is the "tendency to explain behavior on a basis of organic impulse in fundamental separation from external influences" (9, p. 311). In considering this criticism, we must raise two separate and distinct questions: (*a*) is such a tendency necessarily implied by a concept such as perceptual defense (or, in general, by the hypothesis that needs may influence perception) and (*b*) has there, in point of actual fact, been such a tendency on the part of certain investigators? I believe the answer to the first question is no, the answer to the second question is yes.

Operationally and empirically, there is nothing in the concept of perceptual defense which entails an interpretation of behavior on the basis of organic impulses in separation from external influences. As was pointed out above, defense referred to quantitative variation in recognition thresholds. Raised thresholds for certain types of stimuli were designated as defensive. This means that conditions of the organism may modulate or affect the perceptual process and, in this case, cause the final correct discrimination to be delayed. In the same context, we have often spoken of the *joint determination* of perceptual discrimination by sensory and directive factors (4, 16, 22).[5] Such an analysis cannot reasonably be construed as representing an interpretation of behavior exclusively in terms of motivational determinants without regard to external (stimulus) determinants. Indeed, the very operations of the experiments in which perceptual defense was believed to have been demonstrated—the measurement of discrimination thresholds for certain classes of stimuli as a function of such variables as exposure time and/or intensity of illumination—clearly disposes of the allegation of internalism.

It is true, however, that some investigators have, in the interpretation of their results, shown a "projective bias," i.e., they have neglected to carry out a full and precise analysis of the stimulus conditions and response dispositions on which their inferences about perceptual discrimination were based.[6] Such a projective bias easily leads the investigator to ascribe to the operation of special motivational mechanisms observed variations in perception which can be economically interpreted in terms of more general principles of discrimination (cf. 7, 18). There is nothing about the hypothesis that needs may influence perception which, of necessity, leads to a projective bias in the interpretation of experimental data. In each experimental investigation, it is an empirical question whether and to what extent the manipulation of motivational variables results in significant effects upon perceptual response dispositions.

The final argument in Howie's critique attempts to interpret the hypothesis of perceptual defense as an im-

[5] Cf. also the distinction made by Krech and Crutchfield (10) between structural and functional factors.

[6] For a discussion of an operational analysis of experiments on motivation and perception see (17).

plicit endorsement of Freud's principle of economy—that the organism has a basic drive to protect itself against stimulation. The chain of reasoning which leads to this rather startling conclusion is somewhat as follows: (*a*) a "suspicion" that the principle of perceptual defense is an offspring of the economy principle, (*b*) *ergo*, the principle of perceptual defense partakes of the same difficulties as does the principle of economy. By a series of assumptions and extrapolations which leave one rather breathless the hypothesis of perceptual defense is made tantamount to the theory of the death instinct. And a hypothesis which was designed to account for certain variations in recognition thresholds now has the consequence that "adjustment, intelligent behavior, and achievement become but 'more complicated and circuitous routes to the attainment of the goal of death' [*sic*!]" (9, p. 314).

It seems hardly necessary to try to refute this argument in detail. I would venture only the following remarks.

1. Howie's suspicions about the ancestry of perceptual defense were unfounded; he worried needlessly.

2. Although I am as much opposed to some of the excesses of Freudian theory as anyone else, I would hope that it is possible today to subject concepts associated in one way or another with psychoanalysis to experimental tests without arousing irrelevant emotional reactions to the entire body of Freudian doctrine.

3. The evaluation of concepts in terms of the motives which allegedly led to their introduction cannot but result in regrettable misunderstandings and futile debate. Concepts must stand or fall in their own right, whatever the reasons, motives, or hunches which led someone to think of them. And in any event, however astute the critic, he is as likely as not to fail in his attempts at reconstructing the thought processes of the theorists with whom he wishes to disagree.

SUMMARY AND CONCLUSIONS

The concept of perceptual defense was formulated to account for a specific set of empirical data—raised recognition thresholds for stimuli believed to be anxiety-arousing. The concept was not, and cannot be, regarded as an irreducible explanatory principle. To be maintained, it would have to be anchored in antecedent and consequent conditions, and the mechanisms mediating defense would have to be specified and related to other principles of perceptual functioning. Experimental and theoretical reanalysis suggests that perceptual defense need not, at least for the present, be regarded as a special principle of perception. The data to which it refers can be conceptualized in terms of more general principles.

The concept cannot, however, legitimately be rejected on purely logical, epistemological, or metaphysical grounds.

The logical paradox of "knowing in order not to know" disappears when it is recognized that more than one class of discriminatory responses may occur in response to the same stimulus. Such different discriminations need not have the same thresholds and may under some conditions inhibit each other.

It is inappropriate to evaluate hypotheses concerning perceptual selectivity in terms of preconceptions about a required degree of correspondence between the "real" and the perceptual. How well perceptual discrimination corresponds, in any given situation, with other measurements of the stimulus is an empirical question.

The hypothesis that motivational factors may influence perception (e.g., result in delay of recognition) does not entail neglect of stimulus determinants or a disregard for the *S*'s verbal and

motor response dispositions. Such a hypothesis merely focuses attention on the contribution of additional variables which in any given situation may or may not be significant.

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RATIONAL BEHAVIOR AND ECONOMIC BEHAVIOR

GEORGE KATONA

Survey Research Center, University of Michigan

While attempts to penetrate the boundary lines between psychology and sociology have been rather frequent during the last few decades, psychologists have paid little attention to the problems with which another sister discipline, economics, is concerned. One purpose of this paper is to arouse interest among psychologists in studies of economic behavior. For that purpose it will be shown that psychological principles may be of great value in clarifying basic questions of economics and that the psychology of habit formation, of motivation, and of group belonging may profit from studies of economic behavior.

A variety of significant problems, such as those of the business cycle or inflation, of consumer saving or business investment, could be chosen for the purpose of such demonstration. This paper, however, will be concerned with the most fundamental assumption of economics, the principle of rationality. In order to clarify the problems involved in this principle, which have been neglected by contemporary psychologists, it will be necessary to contrast the most common forms of methodology used in economics with those employed in psychology and to discuss the role of empirical research in the social sciences.

Theory and Hypotheses

Economic theory represents one of the oldest and most elaborate theoretical structures in the social sciences. However, dissatisfaction with the achievements and uses of economic theory has grown considerably during the past few decades on the part of economists who are interested in what actually goes on in economic life. And yet leading sociologists and psychologists have recently declared "Economics is today, in a theoretical sense, probably the most highly elaborated, sophisticated, and refined of the disciplines dealing with action" (15, p. 28).[1]

To understand the scientific approach of economic theorists, we may divide them into two groups. Some develop an a priori system from which they deduce propositions about how people *should* act under certain assumptions. Assuming that the sole aim of businessmen is profit maximization, these theorists deduce propositions about marginal revenues and marginal costs, for example, that are not meant to be suited for testing. In developing formal logics of economic action, one of the main considerations is elegance of the deductive system, based on the law of parsimony. A wide gap separates these theorists from economic research of an empirical-statistical type which registers what they call aberrations or deviations, due to human frailty, from the norm set by theory.

A second group of economic theorists adheres to the proposition that it is the main purpose of theory to provide hypotheses that can be tested. This group acknowledges that prediction of future events represents the most stringent test of theory. They argue, however, that reality is so complex that it is necessary to begin with simplified propositions and models which are known to be unreal

[1] The quotation is from an introductory general statement signed by T. Parsons, E. A. Shils, G. W. Allport, C. Kluckhohn, H. A. Murray, R. R. Sears, R. C. Sheldon, S. A. Stouffer, and E. C. Tolman. The term "action" is meant to be synonymous with "behavior."

and not testable.[2] Basic among these propositions are the following three which traditionally have served to characterize the economic man or the rational man:

1. The principle of complete information and foresight. Economic conditions —demand, supply, prices, etc.—are not only given but also known to the rational man. This applies as well to future conditions about which there exists no uncertainty, so that rational choice can always be made. (In place of the assumption of certainty of future developments, we find nowadays more frequently the assumption that risks prevail but the probability of occurrence of different alternatives is known; this does not constitute a basic difference.)

2. The principle of complete mobility. There are no institutional or psychological factors which make it impossible, or expensive, or slow, to translate the rational choice into action.

3. The principle of pure competition. Individual action has no great influence on prices because each man's choice is independent from any other person's choice and because there are no "large" sellers or buyers. Action is the result of individual choice and is not group-determined.

Economic theory is developed first under these assumptions. The theorists then introduce changes in the assumptions so that the theory may approach reality. One such step consists, for instance, of introducing large-scale producers, monopolists, and oligopolists, another of introducing time lags, and still another of introducing uncertainty about the probability distribution of future events. The question raised in each case is this: Which of the original propositions needs to be changed, and in what way, in view of the new assumptions?

The fact that up to now the procedure of gradual approximation to reality has not been completely successful does not invalidate the method. It must also be acknowledged that propositions were frequently derived from unrealistic economic models which were susceptible to testing and stimulated empirical research. In this paper we shall point to a great drawback of this method of starting out with a simplified a priori system and making it gradually more complex and more real—by proceeding in this way one tends to lose sight of important problems and to disregard them.

The methods most commonly used in psychology may appear at first sight to be quite similar to the methods of economics which have just been described. Psychologists often start with casual observations, derive from them hypotheses, test those through more systematic observations, reformulate and revise their hypotheses accordingly, and test them again. The process of hypotheses-observations-hypotheses-observations often goes on with no end in sight. Differences from the approach of economic theory may be found in the absence in psychological research of detailed systematic elaboration prior to any observation. Also, in psychological research, findings and generalizations in one field of behavior are often considered as hypotheses in another field of behavior. Accordingly, in analyzing economic be-

[2] A variety of methods used in economic research differ, of course, from those employed by the two groups of economic theorists. Some research is motivated by dissatisfaction with the traditional economic theory; some is grounded in a systematization greatly different from traditional theory (the most important example of such systematization is national income accounting); some research is not clearly based on any theory; finally, some research has great affinity with psychological and sociological studies.

havior[3] and trying to understand rationality, psychologists can draw on (*a*) the theory of learning and thinking, (*b*) the theory of group belonging, and (*c*) the theory of motivation. This will be done in this paper.

HABITUAL BEHAVIOR AND GENUINE DECISION MAKING

In trying to give noneconomic examples of "rational calculus," economic theorists have often referred to gambling. From some textbooks one might conclude that the most rational place in the world is the Casino in Monte Carlo where odds and probabilities can be calculated exactly. In contrast, some mathematicians and psychologists have considered scientific discovery and the thought processes of scientists as the best examples of rational or intelligent behavior.[4] An inquiry about the possible contributions of psychology to the analysis of rationality may then begin with a formulation of the differences between (*a*) associative learning and habit formation and (*b*) problem solving and thinking.

The basic principle of the first form of behavior is repetition. Here the argument of Guthrie holds: "The most certain and dependable information concerning what a man will do in any situation is information concerning what he did in that situation on its last occurrence" (4, p. 228). This form of behavior depends upon the frequency of repetition as well as on its recency and on the success of past performances. The origins of habit formation have been demonstrated by experiments about learning nonsense syllables, lists of words, mazes, and conditioned responses. Habits thus formed are to some extent automatic and inflexible.

In contrast, problem-solving behavior has been characterized by the arousal of a problem or question, by deliberation that involves reorganization and "direction," by understanding of the requirements of the situation, by weighing of alternatives and taking their consequences into consideration and, finally, by choosing among alternative courses of action.[5] Scientific discovery is not the only example of such procedures; they have been demonstrated in the psychological laboratory as well as in a variety of real-life situations. Problem solving results in action which is new rather than repetitive; the actor may have never behaved in the same way before and may not have learned of any others having behaved in the same way.

Some of the above terms, defined and analyzed by psychologists, are also being used by economists in their discussion of rational behavior. In discussing, for example, a manufacturer's choice between erecting or not erecting a new factory, or raising or not raising his prices or output, reference is usually made to deliberation and to taking the consequences of alternative choices into consideration. Nevertheless, it is not justified to identify problem-solving behavior with rational behavior. From the point of view of an outside observer,

[3] The expression "economic behavior" is used in this paper to mean behavior concerning economic matters (spending, saving, investing, pricing, etc.). Some economic theorists use the expression to mean the behavior of the "economic man," that is, the behavior postulated in their theory of rationality.

[4] Reference should be made first of all to Max Wertheimer who in his book *Productive Thinking* (17) uses the terms "sensible" and "intelligent" rather than "rational." Since we are mainly interested here in deriving conclusions from the psychology of thinking, the discussion of psychological principles will be kept extremely brief (see 6 and 8, Chap. 3, 4).

[5] Cf. the following statement by a leading psychoanalyst: "Rational behavior is behavior that is effectively guided by an understanding of the situation to which one is reacting" (3, p. 16). French adds two steps that follow the choice between alternative goals, namely, commitment to a goal and commitment to a plan to reach a goal.

habitual behavior may prove to be fully rational or the most appropriate way of action under certain circumstances. All that is claimed here is that the analysis of two forms of behavior—habitual versus genuine decision making—may serve to clarify problems of rationality. We shall proceed therefore by deriving six propositions from the psychological principles. To some extent, or in certain fields of behavior, these are findings or empirical generalizations; to some extent, or in other fields of behavior, they are hypotheses.

1. Problem-solving behavior is a relatively rare occurrence. It would be incorrect to assume that everyday behavior consistently manifests such features as arousal of a problem, deliberation, or taking consequences of the action into consideration. Behavior which does not manifest these characteristics predominates in everyday life and in economic activities as well.

2. The main alternative to problem-solving behavior is not whimsical or impulsive behavior (which was considered the major example of "irrational" behavior by nineteenth century philosophers). When genuine decision making does not take place, habitual behavior is the most usual occurrence: people act as they have acted before under similar circumstances, without deliberating and choosing.

3. Problem-solving behavior is recognized most commonly as a deviation from habitual behavior. Observance of the established routine is abandoned when in driving home from my office, for example, I learn that there is a parade in town and choose a different route, instead of automatically taking the usual one. Or, to mention an example of economic behavior: Many businessmen have rules of thumb concerning the timing for reorders of merchandise; yet sometimes they decide to place new orders even though their inventories have not reached the usual level of depletion (for instance, because they anticipate price increases), or not to order merchandise even though that level has been reached (because they expect a slump in sales).

4. Strong motivational forces—stronger than those which elicit habitual behavior—must be present to call forth problem-solving behavior. Being in a "crossroad situation," facing "choice points," or perceiving that something new has occurred are typical instances in which we are motivated to deliberate and choose. Pearl Harbor and the Korean aggression are extreme examples of "new" events; economic behavior of the problem-solving type was found to have prevailed widely after these events.

5. Group belonging and group reinforcement play a substantial role in changes of behavior due to problem solving. Many people become aware of the same events at the same time; our mass media provide the same information and often the same interpretation of events to groups of people (to businessmen, trade union members, sometimes to all Americans). Changes in behavior resulting from new events may therefore occur among very many people at the same time. Some economists (for instance, Lord Keynes, see 9, p. 95) argued that consumer optimism and pessimism are unimportant because usually they will cancel out; in the light of sociopsychological principles, however, it is probable, and has been confirmed by recent surveys, that a change from optimistic to pessimistic attitudes, or vice versa, sometimes occurs among millions of people at the same time.

6. Changes in behavior due to genuine decision making will tend to be substantial and abrupt, rather than small and gradual. Typical examples of action that results from genuine decisions are cessation of purchases or buying waves, the shutting down of plants or the build-

ing of new plants, rather than an increase or decrease of production by 5 or 10 per cent.[6]

Because of the preponderance of individual psychological assumptions in classical economics and the emphasis placed on group behavior in this discussion, the change in underlying conditions which has occurred during the last century may be illustrated by a further example. It is related—the author does not know whether the story is true or fictitious—that the banking house of the Rothschilds, still in its infancy at that time, was one of the suppliers of the armies of Lord Wellington in 1815. Nathan Mayer Rothschild accompanied the armies and was present at the Battle of Waterloo. When he became convinced that Napoleon was decisively defeated, he released carrier pigeons so as to transmit the news to his associates in London and reverse the commodity position of his bank. The carrier pigeons arrived in London before the news of the victory became public knowledge. The profits thus reaped laid, according to the story, the foundation to the outstanding position of the House of Rothschild in the following decades.

The decision to embark on a new course of action because of new events was then made by one individual for his own profit. At present, news of a battle, or of change of government, or of rearmament programs, is transmitted in short order by press and radio to the public at large. Businessmen—the manufacturers or retailers of steel or clothing, for instance—usually receive the same news about changes in the price of raw materials or in demand, and often consult with each other. Belonging to the same group means being subject to similar stimuli and reinforcing one another in making decisions. Acting in the same way as other members of one's group or of a reference group have acted under similar circumstances may also occur without deliberation and choice. New action by a few manufacturers will, then, frequently or even usually not be compensated by reverse action on the part of others. Rather the direction in which the economy of an entire country moves—and often the world economy as well—will tend to be subject to the same influences.

After having indicated some of the contributions which the application of certain psychological principles to economic behavior may make, we turn to contrasting that approach with the traditional theory of rationality. Instead of referring to the formulations of nineteenth century economists, we shall quote from a modern version of the classical trend of thought. The title of a section in a recent article by Kenneth J. Arrow is "The Principle of Rationality." He describes one of the criteria of rationality as follows: "We can imagine the individual as listing, once and for all, all conceivable consequences of his actions in order of his preference for them" (1, p. 135). We are first concerned with the expression "all conceivable consequences." This expression seems to contradict the principle of selectivity of human behavior. Yet habitual behavior is highly selective since it is based on (repeated) past experience, and problem-solving behavior likewise is highly selective since reorganization is subject to a certain direction instead of consisting of trial (and error) regarding all possible avenues of action.

Secondly, Arrow appears to identify rationality with consistency in the sense of repetition of the same choice. It is part and parcel of rational behavior,

[6] Some empirical evidence supporting these six propositions in the area of economic behavior has been assembled by the Survey Research Center of the University of Michigan (see 8 and also 7).

according to Arrow, that an individual "makes the same choice each time he is confronted with the same set of alternatives" (1, p. 135).[7] Proceeding in the same way on successive occasions appears, however, a characteristic of habitual behavior. Problem-solving behavior, on the other hand, is flexible. Rationality may be said to reflect adaptability and ability to act in a new way when circumstances demand it, rather than to consist of rigid or repetitive behavior.

Thirdly, it is important to realize the differences between the concepts action, decision, and choice. It is an essential feature of the approach derived from considering problem-solving behavior that there is action without deliberate decision and choice. It then becomes one of the most important problems of research to determine under what conditions genuine decision and choice occur prior to an action. The three concepts are, however, used without differentiation in the classical theory of rationality and also, most recently, by Parsons and Shils. According to the theory of these authors, there are "five discrete choices (explicit or implicit) which every actor makes before he can act" (15, p. 78); before there is action "a decision must always be made (explicitly or implicitly, consciously or unconsciously)" (15, p. 89).

There exists, no doubt, a difference in terminology, which may be clarified by mentioning a simple case: Suppose my telephone rings; I lift the receiver with my left hand and say, "Hello." Should we then argue that I made several choices, for instance, that I decided not to lift the receiver with my right hand and not to say "Mr. Katona speaking"? According to our use of the terms decision and choice, my action was habitual and did not involve "taking consequences into consideration."[8] Parsons and Shils use the terms decision and choice in a different sense, and Arrow may use the terms "all conceivable consequences" and "same set of alternatives" in a different sense from the one employed in this paper. But the difference between the two approaches appears to be more far-reaching. By using the terminology of the authors quoted, and by constructing a theory of rational action on the basis of this terminology, fundamental problems are disregarded. If every action by definition presupposes decision making, and if the malleability of human behavior is not taken into consideration, a one-sided theory of rationality is developed and empirical research is confined to testing a theory which covers only some of the aspects of rationality.

This was the case recently in experiments devised by Mosteller and Nogee. These authors attempt to test basic assumptions of economic theory, such as the rational choice among alternatives, by placing their subjects in a gambling situation (a variation of poker dice) and compelling them to make a decision, namely, to play or not to play against the experimenter. Through their experiments the authors prove that "it is feasible to measure utility experimentally" (14, p. 403) but they do not shed light on the conditions under which rational

[7] In his recent book Arrow adds after stating that the economic man "will make the same decision each time he is faced with the same range of alternatives": "The ability to make consistent decisions is one of the symptoms of an integrated personality" (2, p. 2).

[8] If I have reason not to make known that I am at home, I may react to the ringing of the telephone by fright, indecision, and deliberation (should I lift the receiver or let the telephone ring?) instead of reacting in the habitual way. This is an example of problem-solving behavior characterized as deviating from habitual behavior. The only example of action mentioned by Parsons and Shils, "a man driving his automobile to a lake to go fishing," may be habitual or may be an instance of genuine decision making.

behavior occurs or on the inherent features of rational behavior. Experiments in which making a choice among known alternatives is prescribed do not test the realism of economic theory.

MAXIMIZATION

Up to now we have discussed only one central aspect of rationality—means rather than ends. The end of rational behavior, according to economic theory, is maximization of profits in the case of business firms and maximization of utility in the case of people in general.

A few words, first, on maximizing profits. This is usually considered the simpler case because it is widely held (*a*) that business firms are in business to make profits and (*b*) that profits, more so than utility, are a quantitative, measurable concept.

When empirical research, most commonly in the form of case studies, showed that businessmen frequently strove for many things in addition to profits or in place of profits, most theorists were content with small changes in their systems. They redefined profits so as to include long-range profits and what has been called nonpecuniary or psychic profits. Striving for security or for power was identified with striving for profits in the more distant future; purchasing goods from a high bidder who was a member of the same fraternity as the purchaser, rather than from the lowest bidder—to cite an example often used in textbooks—was thought to be maximizing of nonpecuniary profits. Dissatisfaction with this type of theory construction is rather widespread. For example, a leading theorist wrote recently:

> If *whatever* a business man does is explained by the principle of profit maximization—because he does what he likes to do, and he likes to do what maximizes the sum of his pecuniary and non-pecuniary profits—the analysis acquires the character of a system of definitions and tautologies, and loses much of its value as an explanation of reality (13, p. 526).

The same problem is encountered regarding maximization of utility. Arrow defines rational behavior as follows: ". . . among all the combinations of commodities an individual can afford, he chooses that combination which maximizes his utility or satisfaction" (1, p. 135) and speaks of the "traditional identification of rationality with maximization of some sort" (2, p. 3). An economic theorist has recently characterized this type of definition as follows:

> The statement that a person seeks to maximize utility is (in many versions) a tautology: it is impossible to conceive of an observational phenomenon that contradicts it. . . . What if the theorem is contradicted by observation: Samuelson says it would not matter much in the case of utility theory; I would say that it would not make the slightest difference. For there is a free variable in his system: the tastes of consumers. . . . Any contradiction of a theorem derived from utility theory can always be attributed to a change of tastes, rather than to an error in the postulates or logic of the theory (16, pp. 603 f.).[9]

What is the way out of this difficulty? Can psychology, and specifically the psychology of motivation, help? We may begin by characterizing the prevailing economic theory as a single-motive theory and contrast it with a theory of multiple motives. Even in case of a single decision of one individual, multiplicity of motives (or of vectors or forces in the field), some reinforcing one another and some conflicting with one another, is the rule rather than the exception. The motivational patterns prevailing among different individuals making the same decision need not be the same; the motives of the same individual who is in the same external situation at different times may likewise

[9] The quotation refers specifically to Samuelson's definition but also applies to that of Arrow.

differ. This approach opens the way (*a*) for a study of the relation of different motives to different forms of behavior and (*b*) for an investigation of changes in motives. Both problems are disregarded by postulating a single-motive theory and by restricting empirical studies to attempts to confirm or contradict that theory.

The fruitfulness of the psychological approach may be illustrated first by a brief reference to business motivation. We may rank the diverse motivational patterns of businessmen by placing the striving for high immediate profits (maximization of short-run profits, to use economic terminology; charging whatever the market can bear, to use a popular expression) at one extreme of the scale. At the other extreme we place the striving for prestige or power. In between we discern striving for security, for larger business volume, or for profits in the more distant future. Under what kinds of business conditions will motivational patterns tend to conform with the one or the other end of the scale? Preliminary studies would seem to indicate that the worse the business situation is, the more frequent is striving for high immediate profits, and the better the business situation is, the more frequent is striving for nonpecuniary goals (see 8, pp. 193–213).

Next we shall refer to one of the most important problems of consumer economics as well as of business-cycle studies, the deliberate choice between saving and spending. Suppose a college professor receives a raise in his salary or makes a few hundred extra dollars through a publication. Suppose, furthermore, that he suggests thereupon to his wife that they should buy a television set, while the wife argues that the money should be put in the bank as a reserve against a "rainy day." Whatever the final decision may be, traditional economic theory would hold that the action which gives the greater satisfaction was chosen. This way of theorizing is of little value. Under what conditions will one type of behavior (spending) and under what conditions will another type of behavior (saving) be more frequent? Psychological hypotheses according to which the strength of vectors is related to the immediacy of needs have been put to a test through nationwide surveys over the past six years.[10] On the basis of survey findings the following tentative generalization was established: Pessimism, insecurity, expectation of income declines or bad times in the near future promote saving (putting the extra money in the bank), while optimism, feeling of security, expectation of income increases, or good times promote spending (buying the television set, for instance).

Psychological hypotheses, based on a theory of motivational patterns which change with circumstances and influence behavior, thus stimulated empirical studies. These studies, in turn, yielded a better understanding of past developments and also, we may add, better predictions of forthcoming trends than did studies based on the classical theory (see footnote 10). On the other hand, when conclusions about utility or rationality were made on an a priori basis, researchers lost sight of important problems.[11]

[10] In the Surveys of Consumer Finances, conducted annually since 1946 by the Survey Research Center of the University of Michigan for the Federal Reserve Board and reported in the *Federal Reserve Bulletin*. See also 8 and a forthcoming publication of the Survey Research Center on consumer buying and inflation during 1950–52.

[11] It should not be implied that the concepts of utility and maximization are of no value for empirical research. Comparison between maximum utility as determined from the vantage point of an observer with the pattern of goals actually chosen (the "subjective maximum"), which is based on insufficient information, may be useful. Similar considerations apply to such newer concepts as "minimizing regrets" and the "minimax."

DIMINISHING UTILITY, SATURATION, AND ASPIRATION

Among the problems to which the identification of maximizing utility with rationality gave rise, the measurability of utility has been prominent. At present the position of most economists appears to be that while interpersonal comparison of several consumers' utilities is not possible, and while cardinal measures cannot be attached to the utilities of one particular consumer, ordinal ranking of the utilities of each individual can be made. It is asserted that I can always say either that I prefer A to B, or that I am indifferent to having A or B, or that I prefer B to A. The theory of indifference curves is based on this assumption.

In elaborating the theory further, it is asserted that rational behavior consists not only of preferring more of the same goods to less ($2 real wages to $1, or two packages of cigarettes to one package, for the same service performed) but also of deriving diminishing increments of satisfaction from successive units of a commodity.[12] In terms of an old textbook example, one drink of water has tremendous value to a thirsty traveler in a desert; a second, third, or fourth drink may still have some value but less and less so; an nth drink (which he is unable to carry along) has no value at all. A generalization derived from this principle is that the more of a commodity or the more money a person has, the smaller are his needs for that commodity or for money, and the smaller his incentives to add to what he has.

In addition to using this principle of saturation to describe the behavior of the rational man, modern economists applied it to one of the most pressing problems of contemporary American economy. Prior to World War II the American people (not counting business firms) owned about 45 billion dollars in liquid assets (currency, bank deposits, government bonds) and these funds were highly concentrated among relatively few families; most individual families held no liquid assets at all (except for small amounts of currency). By the end of the year 1945, however, the personal liquid-asset holdings had risen to about 140 billion dollars and four out of every five families owned some bank deposits or war bonds. What is the effect of this great change on spending and saving? This question has been answered by several leading economists in terms of the saturation principle presented above. "The rate of saving is . . . a diminishing function of the wealth the individual holds" (5, p. 499) because "the availability of liquid assets raises consumption generally by reducing the impulse to save."[13] More specifically: a person who owns nothing or very little will exert himself greatly to acquire some reserve funds, while a person who owns much will have much smaller incentives to save. Similarly, incentives to increase one's income are said to weaken with the amount of income. In other words, the strength of motivation is inversely correlated with the level of achievement.

In view of the lack of contact between economists and psychologists, it is hardly surprising that economists failed to see the relevance for their postulates of the extensive experimental work performed by psychologists on the problem of levels of aspiration. It is not necessary in this paper to describe these studies in detail. It may suffice to for-

[12] This principle of diminishing utility was called a "fundamental tendency of human nature" by the great nineteenth century economist, Alfred Marshall.

[13] The last quotation is from the publication of the U. S. Department of Commerce, *Survey of Current Business*, May 1950, p. 10. This quotation and several similar ones are discussed in 8, pp. 186 ff.

mulate three generalizations as established in numerous studies of goal-striving behavior (see, for example, 12):

1. Aspirations are not static, they are not established once for all time.
2. Aspirations tend to grow with achievement and decline with failure.
3. Aspirations are influenced by the performance of other members of the group to which one belongs and by that of reference groups.

From these generalizations hypotheses were derived about the influence of assets on saving which differed from the postulates of the saturation theory. This is not the place to describe the extensive empirical work undertaken to test the hypotheses. But it may be reported that the saturation theory was not confirmed; the level-of-aspiration theory likewise did not suffice to explain the findings. In addition to the variable "size of liquid-asset holdings," the studies had to consider such variables as income level, income change, and savings habits. (Holders of large liquid assets are primarily people who have saved a high proportion of their income in the past!) [14]

The necessity of studying the interaction of a great number of variables and the change of choices over time leads to doubts regarding the universal validity of a one-dimensional ordering of all alternatives. The theory of measurement of utilities remains an empty frame unless people's established preferences of *A* over *B* and of *B* over *C* provide indications about their probable future behavior. Under what conditions do people's preferences give us such clues, and under what conditions do they not? If at different times *A* and *B* are seen in different contexts—because of changed external conditions or the acquisition of new experiences—we may have to distinguish among several dimensions.

The problem may be illustrated by an analogy. Classic economic theory postulates a one-dimensional ordering of all alternatives; Gallup asserts that answers to questions of choice can always be ordered on a yes–uncertain (don't know)–no continuum; are both arguments subject to the same reservations? Specifically, if two persons give the same answer to a poll question (e.g., both say "Yes, I am for sending American troops to Europe" or "Yes, I am for the Taft-Hartley Act") may they mean different things so that their identical answers do not permit any conclusions about the similarity of their other attitudes and their behavior? Methodologically it follows from the last argument that yes-no questions need to be supplemented by open-ended questions to discern differences in people's level of information and motivation. It also follows that attitudes and preferences should be ascertained through a multi-question approach (or scaling) which serves to determine whether one or several dimensions prevail.

ON THEORY CONSTRUCTION

In attempting to summarize our conclusions about the respective merits of different scientific approaches, we might quote the conclusions of Arrow which he formulated for social science in general rather than for economics:

> To the extent that formal theoretical structures in the social sciences have not been based on the hypothesis of rational behavior, their postulates have been developed in a manner which we may term *ad hoc*. Such propositions . . . depend, of course, on the investigator's intuition and common sense (1, p. 137).

The last sentence seems strange indeed. One may argue the other way around and point out that such propositions as "the purpose of business is

[14] The empirical work was part of the economic behavior program of the Survey Research Center under the direction of the author. See (8) and also (10) and (11).

to make profits" or "the best businessman is the one who maximizes profits" are based on intuition or supposed common sense, rather than on controlled observation. The main problem raised by the quotation concerns the function of empirical research. There exists an alternative to developing an axiomatic system into a full-fledged theoretical model in advance of testing the theory through observations. Controlled observations should be based on hypotheses, and the formulation of an integrated theory need not be delayed until all observations are completed. Yet theory construction is part of the process of hypothesis-observation-revised hypothesis and prediction-observation, and systematization should rely on some empirical research. The proximate aim of scientific research is a body of empirically validated generalizations and not a theory that is valid under any and all circumstances.

The dictum that "theoretical structures in the social sciences must be based on the hypothesis of rational behavior" presupposes that it is established what rational behavior is. Yet, instead of establishing the characteristics of rational behavior a priori, we must first determine the conditions a_1, b_1, c_1 under which behavior of the type x_1, y_1, z_1 and the conditions a_2, b_2, c_2 under which behavior of the type x_2, y_2, z_2 is likely to occur. Then, if we wish, we may designate one of the forms of behavior as rational. The contributions of psychology to this process are not solely methodological; findings and principles about noneconomic behavior provide hypotheses for the study of economic behavior. Likewise, psychology can profit from the study of economic behavior because many aspects of behavior, and among them the problems of rationality, may be studied most fruitfully in the economic field.

This paper was meant to indicate some promising leads for a study of rationality, not to carry such study to its completion. Among the problems that were not considered adequately were the philosophical ones (rationality viewed as a value concept), the psychoanalytic ones (the relationships between rational and conscious, and between irrational and unconscious), and those relating to personality theory and the roots of rationality. The emphasis was placed here on the possibility and fruitfulness of studying forms of rational behavior, rather than the characteristics of *the* rational man. Motives and goals that change with and are adapted to circumstances, and the relatively rare but highly significant cases of our becoming aware of problems and attempting to solve them, were found to be related to behavior that may be called truly rational.

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THINKING CONCEPTUALIZED IN TERMS OF INTERACTING MOMENTS [1]

PAUL McREYNOLDS

Veterans Administration, Palo Alto

One of the most striking defining features of the psychology of man is that he is a thinking creature. The ever-flowing stream of mental activity—the stream of thought, as James called it, is a most fundamental fact of our science. As Hebb (5) has pointed out, the problem of the thinking process is of pre-eminent importance, and must be dealt with if a number of subsidiary problems are to be adequately handled.

This paper presents a broad and schematic formulation concerning the thinking process. While it is highly speculative and does not touch upon a number of significant problems involved in thinking, it can be argued that all we are equipped to do in this area at the present time is to speculate—as cleverly and hopefully as we can. In a matter as important as that under consideration a broad freedom in hypothesizing appears permissible, providing that it is labeled as speculation.

Transient Functions

By *transient functions* I refer to all those mental phenomena which have a phenomenally transient existence, such as thinking, feeling, imagining, fantasying, and dreaming. Transient functions have the characteristic that they occur along a physical time dimension, so that they may be likened phenomenally to a "stream of consciousness."

We assume that underlying such mental activity is neural activity, which represents it in an invariant manner. We therefore are interested in the nature of this neural activity. We assume, on introspective grounds, that the content of the mental activity undergoes relatively constant change, and consequently we assume a relatively constant change in the underlying neural activity.

A question immediately arises: does this changing neural activity consist of changes in terms of which *specific* neural units are being discharged, or does it consist rather of changes in the *pattern* of pertinent neural activity? The first alternative implies that the discharge of a given neural unit would always indicate a given mental content; the second implies that the discharge of a given neural unit would not indicate a given mental content, but rather that its "meaning" would be a function of the pattern of neural discharges of which it was a part.

I have chosen to follow up certain implications of the second alternative. My reasons for assuming the patterning possibility are discussed elsewhere (16). Briefly, they are based upon the argument that a pattern system would be more efficient than a specific unit system for transient functions. I do not postulate any actual likeness between the neural pattern and the percept it represents. The neural pattern would be symbolic in representation and not isomorphic, topologically or otherwise.

One important implication of the pattern approach should be noted immedi-

[1] From the Veterans Administration Hospital, Palo Alto, California. This paper is a revision of a talk delivered at a Research Conference of the Bay Area Clinical Psychologists, October 4, 1951, Palo Alto. Some of the ideas presented were developed originally during discussions with Fred Attneave, who, along with Gregory Bateson and C. L. Winder, read the manuscript and made a number of helpful suggestions.

ately. If the *same* neural units may express different thought contents at different times, which is the essence of a pattern system, then these same neural units cannot also be postulated to underlie memory, i.e., those neurons whose discharge would imply a given mental content cannot be the *same* neurons which somehow maintain such of this content as may be remembered, for in the former case the neural discharge would mean different things at different times, whereas in the latter case it would be required that the neuron somehow carry relatively permanently a given meaning. The conceptualization being presented therefore supposes an anatomically, though not functionally, separate memorial system. This separation, it may be noted, is not made in Hebb's (5) theory: in his theory the neural basis of transient functions consists essentially of the discharge of those same neurons which carry the given data as memory.

The neural discharge pattern postulated to underlie transient functions may be viewed in either of two ways: first, as a succession of different discharge patterns occurring periodically or quasi-periodically; or, second, as a continuously changing discharge pattern. The first possibility implies that *changes* in the pattern would occur *between* the successive pattern discharges, whereas the second implies that the effective discharges would be continuous. It is possible that our choice from these alternatives is simply one of theoretical convenience, but it does appear that the first possibility has a certain logical advantage. If pattern discharges are to have specific meanings and effects—on behavior, memory, or consciousness—they must be specific patterns, i.e., there must be no pattern change during the discharge. This is merely another way of stating the first alternative above.

We assume, then, a succession of neural discharge patterns as underlying transient mental functions. I will refer to these successive patterns as *moments* (*M*'s). This is after the work of Stroud (20, 21), though my usage of the word is slightly different from his. The phenomenal counterpart of a *M* may be termed a *percept*, and *moment sequence* will be used to refer to any number of *M*'s which occur successively. The phenomenal counterpart on an *M* sequence would be, under appropriate conditions, a "stream of thought."

The hypothesis that thinking somehow consists of a succession of discrete "pieces" is not a new one. Bain (9, p. 245) and James (9) may be referred to in this connection. More recently a "moment function hypothesis" has been stated by Stroud (20), and McCulloch and Pitts (15) have independently developed, in a different context, what is essentially a discontinuity hypothesis. Also, Walter (24) has presented a bold theory of transient functions based largely upon a television analogy. All these persons have developed important and stimulating points of view. While my interpretation differs in many respects from theirs, I wish to acknowledge my strong indebtedness to them.

EMPIRICAL CONSIDERATIONS

Sufficient experimental data are not yet available to provide crucial tests of the points so far stated. There are certain findings, however, which are in accord with the hypothesis, i.e., which suggest that the brain works *as if* it were like our model, and some of these may be noted now.

Perhaps the most obvious suggestive evidence that transient mental functions occur discretely is the rhythmic, quasi-periodic nature of the EEG. There are, of course, various electrical rhythms in the brain involving complicated phase relationships and harmonics, although by no means all of the electrical activity

of the brain is periodic. Alpha is probably the best-known rhythm, though it is by no means certain that the various manifestations of 10-per-sec. activity which are termed alpha all have the same function or origin. Further, it appears that at least in certain areas alpha may be a complex with three different components (26). The suggestion here is that at least some of the alpha activity is intimately related to the postulated discrete, successive *M*'s of thought.[2]

It follows, as Walter (24) has pointed out in a different context, that the interval between *M*'s should typically approximate .1 sec. One should, therefore, expect to find in the literature certain pertinent time values of .1 sec. Several of these may be mentioned briefly. (For other examples see the sections on Voluntary Action and on Memory and Learning in this paper.)

(*a*) Dodge (4) in 1907 postulated the necessity of a clearing-up process in visual perception which precludes a succession of adequate visual fixations under .1 sec. each. (*b*) Pillsbury (27, p. 696) in 1913 concluded that the maximum rate of shift of attention is once every .1 sec. (*c*) Stein (27, p. 689) in 1928 reported that a word tends to be perceived as the same regardless of the order in which the letters are tachistoscopically presented provided the total exposure time does not exceed .1 sec. (*d*) The brightness enhancement of a flickering light is maximum when the pulsation rate is in the neighborhood of 10 per sec. (2, p. 974).

These data should not be interpreted to suggest that the brain cannot deal with stimuli or changes in stimuli of less than .1-sec. duration, as Walter (24) appears to suggest. McCulloch (15), likewise, hypothesizes .1 sec. as a scanning interval, and thus implies that the brain (at least with regard to auditory and visual stimuli) effectively handles only stimulus changes occurring every .1 sec. or more. I am not absolutely certain that I have interpreted these theorists correctly, but in any event it is to be noted that stimulus changes involving much less than .1 sec. may be reacted to. This statement is confirmed by work in stroboscopic motion, in flicker, and in flutter-fusion, in all of which changes over time values of much less than .1 sec. may influence the perception.

The situation, then, is this: certain mental phenomena appear to be based upon a mechanism with approximately a .1-sec. time unit, whereas certain other data appear to be dealt with in terms of much shorter time units. How may these generalizations be reconciled?

My suggestion is this: that we are here dealing with two different, though closely related problems. I suggest that the dynamics of data at the high level —in terms of conscious perception, awareness, decisions, changes in the train of thought, and conscious discrimination —operate in terms of the successive neurological patterns which I have referred to as *M*'s, the time unit for which approximates .1 sec. I further suggest that there are in the brain analyzing areas, the function of which is to react to changes in sensory data occurring over very brief periods before these data are expressed as *M*'s, and that the integrating time values for these areas may be much less than .1 sec. This

[2] While this interpretation assumes certain EEG waves to be an *index* of neural discharge, it *does not* assume that the observed changes in potential which constitute the EEG are due to aggregates of fiber potential spikes. Rather, it is probable that certain EEG waves represent directly a change in potential (14). A function of this might be to facilitate periodic discharges in certain neural groups. The postulated relation of certain EEG rhythms to *M* discharges might, therefore, be due to the brain waves functioning as a pacemaker.

would permit the changes per se to be represented in *M*'s.

It may be asked: if the alpha rhythm is invoked as a rough index of successive *M* frequency, why is it that alpha is so easily disrupted by sensory stimulation, and what does the disruption mean? The inference that alpha refers to a quasi-periodic transient mental function appears to be contraindicated at the outset by the fact that when transient functions are, in a sense, most pronounced, as when one is giving rapt attention to a sensory stimulus, the alpha disappears.[3]

Let us examine briefly the nature of the sensory input. All, or nearly all, such information is carried on neurons, and there are three possible ways by which information may be expressed by neurons: (*a*) frequency of impulses, (*b*) frequency modulation, and (*c*) specificity of neurons, i.e., *which* neurons are involved. With respect to the first two, the data are meaningful only with regard to some time base. In order for changes in stimulus values to be dealt with accurately, these time bases should be as short as possible. The faster waves which appear when alpha disappears under sensory stimulation may reflect partially the quick, successive integration of the incoming frequency data. It would be a case not so much of alpha being blocked or masked as of its being *succeeded* by wave forms reflecting entirely different functions. This would reconcile the fact that stimulus changes occupying much less than .1 sec. can be perceived with our view that the stream of thought mechanism operates basically upon an approximately 10-per-sec. frequency.

My conjecture is that there exists, somewhere within the brain, a "center" to which all data which become part of the stream of consciousness are sent, and which "directs" our voluntary behavior. The succeeding neural discharge patterns which I have postulated to underlie the stream of thought, and which I have referred to as *M*'s, would occur in this center, which I shall hereafter refer to as the *moment manifold*.[4] The frequency of the *M*'s as stated earlier, is taken to approximate 10 per sec. in the normal, awake adult. When incoming sensory data are not being analyzed (in subjective terms, when no attention is being given to sensory stimuli) those areas of the brain handling such analytic functions would tend to get in harmony with the *M* rhythm.

With regard to the time bases of the

[3] This is the problem faced also, though in different contexts, by McCulloch and by Walter. McCulloch suggests that alpha represents, in certain occipital and temporal areas, the sweep of an alerting pulse involved in the analysis of spatial and temporal patterns, and that the reason alpha apparently disappears, i.e., is not recorded, in the presence of pertinent sensory stimulation, is that the signals caused by sensory data being scanned mask the presence of alpha. In other words, as I understand him, McCulloch believes that alpha does not *really* disappear under sensory stimulation. Walter suggests that alpha indicates, in the space receptor areas of the brain, a mechanism for continuous scanning, somewhat after the manner of television, with a frame frequency of 10 per sec. So long as the field of scan is featureless, the rhythm should be smooth and rhythmic, but in the presence of sensory stimulation of a spatial sort the succession of runs and checks would disturb this characteristic. Insofar as the preliminary analysis in the brain of spatial patterns is concerned, the hypotheses offered by these men are certainly worthy of consideration, though it is difficult to square the facts of perception with a time base as long as .1 sec.

[4] It is tempting to speculate, on the basis of the stimulating work of Penfield and Rasmussen (18) and of Thomson (22), that this "center" may be in the diencephalon. It is to be noted, however, that the hypothesis does not require a "center" in the usual anatomical sense. Proper pacing of noncontiguous discharges could be regulated by a set of neurons providing periodic nonspecific facilitation, making it possible for the *M* manifold to be widely dispersed.

various receiving areas for integrating incoming frequency data, I see no requirement in the present model that these different areas all have the *same* periodicities, or that such periodicities should be invariant. When a perception involves data from several different sensory modalities, however, it would seem that the different periodicities should have some fairly constant ratio to each other, and this expectation may be related speculatively to the large number of harmonics found in the brain (25, 26).

MOMENT INTERACTION

The *M* manifold may be considered in terms of input, output, and intermoment dynamics. The output consists of the periodic *M* discharges. The input, including that from the sensory areas, need not be periodic, or, if some of it is periodic, need not be at *M* frequency, or, if some of it is at *M* frequency, need not be in phase with *M* discharges.

With regard to intermoment dynamics, we postulate that the pattern of each *M* tends to persist in the *M* manifold and to affect the pattern of the next *M*. This is the same postulate I have elsewhere (16) referred to under the heading of "transient traces." It may help us to deal systematically with such matters as the continuity of thought, shifts in the sequence of thought, association of ideas, and rigidity and looseness in thinking.

There are at least two ways in which an *M* pattern might tend to persist, in the sense of affecting the next *M* discharge. (*a*) The neural pattern might tend to continue by means of recurrent circuits, and (*b*) the discharge of a given neural unit in one *M* might affect the excitability curve for the relatively immediate refiring of the same neural circuit. These possibilities are not necessarily mutually exclusive, and each of them could possibly be useful theoretically: the former in helping to explain the tendency of certain features of thought to persist, and the latter in explaining the inability of maintaining exclusive attention on a nonchanging subject for more than a very brief period.

Further conjecture along this line would not be justified here, but I believe there would be no great difficulty in designing a hypothetical model to accomplish any type of persistence that we care to postulate. The real problem is to determine exactly what we want the model to do. For the present let us postulate simply that each *M* pattern tends to persist for a brief time, and that during this time its tendency to persist gradually declines.[5] The time of persistence would have to be great enough to span to the next *M*, so that the greater the persistence, and/or the nearer in time the next *M* (*M* frequency) the greater would be the effect of the persistence in determining the make-up of the succeeding *M*. The apparent simplicity of this postulate is falsely disarming, for all manner of problems arise when one conjectures as to exactly how the persistence of M_1 and the input to the *M* manifold might combine to produce the M_2 output. Without considering these problems here, however, we postulate that the greater the persistence and/or the higher the *M* frequency, the more successive *M*'s will tend to be alike, i.e., the more

[5] The *M* manifold may figuratively be conceptualized as a mosaic undergoing relatively continuous change as a function of (*a*) aperiodic input and/or input at various periodicities, and (*b*) the disappearance of the representation of previous input. The *M*'s may then be conceptualized as periodic patterns of discharge from this mosaic. In terms of analogy, as suggested by Gregory Bateson, one may conceive of a mosaic of lights undergoing relatively continuous change. With a high-speed camera one could take pictures of this mosaic at .1-sec. intervals. These pictures would represent the discharge patterns referred to as *M*'s.

they will tend to have in common. A companion postulate is that *M* frequency and *M* persistence may vary, not only between *S*s, but also within a given *S* at different times.

In addition to *M* interaction in terms of the direct influence of one *M* on the next it is useful to postulate the existence of a mechanism whereby certain relations between two successive *M*'s could themselves be represented as input in a succeeding *M*. For example, M_3 might include some kind of representation of the extent of commonality of M_1 and M_2, or M_3 might include representation of differences in M_1 and M_2. This postulate, too, leads to all manner of difficulty when one attempts to work it out in detail, but I will not discuss those intricacies here. It, or some postulate very much like it, seems to be necessary in order to keep us from getting lost in the fallacy of conceiving of the *M* manifold as a kind of screen upon which appear successive "thoughts" with a "little man in the head" sitting and comfortably viewing the procession.[6] The *M* manifold itself is intended to *be* the "little man," and the model must provide some basis whereby the process of thought itself becomes a part of succeeding thought.

It is suggested also that *M*'s may affect the content of succeeding *M*'s by influencing, to some extent, the input to the *M* manifold from the memorial system. More specifically, it is proposed that there is at least a tendency for the *M*'s to be "filed" in the memorial system, and that in this process certain data, already filed there, become more readily available as new input to the *M* manifold. Among the factors which might determine which of the memorial data would thus be made more available would be the degree of similarity, possibly in terms of common parameters, between the input from the *M* manifold and the various memorial units. Further, I propose, as a postulate of theoretical convenience, that at any given time the extent to which the availability of given memorial data is enhanced in this manner is a function not only of a single preceding *M*, but of the additive effects of at least several preceding *M*'s, their respective influence being a function of their recency.

PERCEPTION OF SIMILARITY

Consider now the problems of conscious discrimination and the perception of the similarity of complex stimuli. In the present model all such perceptions will be considered to be based on successive comparison, i.e., to involve the comparison of successive *M*'s, in still following *M*'s, in terms of their neurological likeness. Note that I am here referring to complex stimuli, i.e., stimuli which have many dimensions. Another type of comparison of stimuli may involve what appears to be a single dimension, such as comparisons of stimuli as to brightness, pitch, hue, etc. This latter type I do not include within the present hypothesis. There is some reason to believe that it may involve an entirely different type of central mechanism, as suggested on logical grounds by James (9, ch. 13) and by Landahl (13). I think it possible that the similarity mechanism for multidimensional stimuli works on some kind of an overlap basis, and that of unidimensional stimuli on some kind of difference, or

[6] An example of this fallacy would be to consider that if successive *M*'s represented the same object just a little different in position in each *M*, as in the successive frames of a motion picture film, the phenomenal perception *ipso facto* would be motion. Such is *not* the case, even aside from the fact that the frequency of the *M*'s, as postulated, is too low to underlie stroboscopic motion. What would result would be simply successive percepts, and nothing else. Motion is itself a perception, and therefore would itself have to be represented in an *M*.

ratio, basis. While I believe that both of these could be fitted into the present model, I will restrict my present discussion to the comparison of complex stimuli.

The overlap just referred to would occur in a sequence of successive *M*'s as the comparative stimuli were being considered.[7] As to just *what* would overlap I can offer only a guess, and my guess is that it would be something like the percentage of parameters in common with the comparative stimuli. In accordance with this view is the finding by Attneave (1) of a high positive correlation between judged similarity of complex verbal stimuli and number of ways the stimuli were conceived as being alike.

The degree of overlap between successive *M*'s would be influenced by the parameters which the comparative stimuli *have* in common, and this in turn presumably would depend to some extent on the familiarity of the *S* with the comparison objects, and the attention he gives them. But it would also depend on *M* frequency and persistence.[8] (For convenience I will hereafter refer to *M* frequency as *M*f and to *M* persistence as *M*p.) In general for an *S* with high *M*f and/or marked *M*p there would tend to be more overlap between successive *M*'s than for *S*s with low *M*f and/or little *M*p.

When an *S* compares two stimuli with regard to their degree of similarity, his judgment is presumably based not only on the perceived likeness between them, but also on his adaptation level for similarity. Use of the concept of adaptation level (AL) in this context, while new, appears to be in accord with the view of Helson (6) that the concept has wide generality. Now $AL_{Sim.}$, in terms of the present model, would be some function of the central tendency of similarity represented in the multitude of past successive pairs of *M*'s. $AL_{Sim.}$ would be high (marked overlap in successive *M*'s) when *M*f is typically high and/or *M*p is typically great; the reverse would also be true. Consequently, even though a given *S*, for a given pair of comparison stimuli, might perceive the same amount of physical likenesses as another *S*, his perception of similarity, founded partly on his $AL_{Sim.}$, would tend to be a function of his typical *M*f and/or *M*p. This variable I have elsewhere (17) discussed under the name of "similarity-standards." It is to be noted that on the basis of the conceptualization proposed here this should tend to be a general trait, i.e., independent, to a large extent, of the type of stimuli being considered (so long as they are complex), since the pertinent variables are not only the particular dimensions of the stimuli, which would differ from stimulus to stimulus, but also *M*f and *M*p, which presumably are independent of the type of stimulus.

The deduction that reactivity to psychological similarity tends to be a general trait, if valid, may have significant implications for understanding individual differences in, and relationships between, all phenomena which involve psychological similarity, such as transfer, satiation and co-satiation, the substitution value of interpolated tasks, the resumption of incompleted tasks, retroactive inhibition, and the degree of heterogeneity of a train of thought.

[7] This is not to imply that comparison of percepts is typically as simple as M_3 for $percept_1$, M_2 for $percept_2$, and M_3 for their comparison, but rather that the situation cannot be simpler, and that this may be the basic paradigm of conscious perceptual discrimination. What usually happens when we compare complex stimuli is that our attention goes repeatedly from one stimulus to the other (8, p. 498), so that a whole complicated *M* sequence would be involved.

[8] The implication of "persistence" in the dynamics of psychological similarity was suggested, in a different context, by Calkins (8, p. 270) in 1892.

VOLUNTARY ACTION

One of the major types of output from the *M* manifold would be voluntary action. There are several areas of evidence which conform to the hypothesis that voluntary behavior is based upon a quasi-periodic function with a typical frequency of approximately 10 per sec.

1. Kibbler, Boreham, and Richter, studying the relation of the alpha rhythm and eye opening in response to an auditory stimulus, reported: "The measurements confirmed that the probability of a response was not randomly distributed in time, but showed peaks and troughs recurring approximately 10 per second and in accurate phase relations with the alpha rhythm. Similar results were obtained for other voluntary movements" (12, p. 371).

2. Reaction time has been widely studied since the last century and yet remains essentially a mystery. For example, for a single visual stimulus, the time lag between the stimulus and the most rapid response averages about .18 sec. Why should it take so long? It takes only about .01 sec. for the impulses to be transmitted to the optic nerve, and only about .01 sec. more for the motor nerve discharge to initiate movement (3).

Two men—independently, so far as I know—have formulated substantially the same hypothesis to explain this problem. Stroud (20), in his pioneering study of periodic phenomena, was struck with the discrepancy between the unpredictability of response to single isolated stimuli and the very precise timing involved in such everyday activities as piano playing and other finely coordinated rhythmic movements. Simple reaction times (*RT*'s) are, after all, really rather astonishingly great. Reaction times of .1 to .2 sec. are, for example, of sizable value in an athletic event such as the hundred-yard dash, or in swerving one's car to prevent an accident. Stroud's hypothesis was essentially that the long *RT*'s are a function of *M* periodicity. Walter (24) has suggested a very similar explanation.

This explanation can be fitted almost directly into the present model. Specifically, the explanation would be as follows. The stimulus, coming at an unpredictable time with respect to the contemporary *M* phase, would occur at any time between, say, plus .09 sec. to minus .01 sec. before a given M_x. This M_x would underlie awareness of the stimulus. But since there can be no change within an *M*, the reaction would be represented in the next *M*, M_y. The total time involved would thus be, for this simplest case, approximately 100 ms. plus the elapsed time before M_x, i.e., between 100 and 200 ms., the exact time being unpredictable in any given case.

Further evidence that voluntary reactions to stimuli are based upon a common periodic mechanism dealing with conscious thought processes is given by the fact that *RT*'s to various types of stimuli tend to be highly intercorrelated (27, p. 337). That the mean *RT*'s differ for different types of stimuli implies that different analytic mechanisms are involved in preparing the sensory data for representation in the *M* manifold, and that these different processes take different lengths of time for the different modalities.

3. Vince (23), in his study of tracking (reactions to continuously changing stimuli), found that *S*s attempting to make the proper movements with a control mechanism in order to follow a moving target tended to respond to the stimuli as a unit when the interval between the stimuli was less than .1 sec. This is in accord with the report of Stetson and McDill who pointed out in 1923 that, with regard to fast movements, "movement elements occurring at the rate of 10 per sec. are the units; at most then

movements can be modified not oftener than ten times in a second" (19, p. 23).

4. The maximum frequency of repetitive movements, such as tapping the finger, is approximately 10 per sec. (27, p. 696).

MEMORY AND LEARNING

Another major type of output from the *M* manifold would be that to the memorial system. The interacting *M*'s conceptualization implies that the units of information to be "filed" are the contents of the *M*'s themselves. A given unit of memory could never be perceived exactly as it was originally, however, because always there would be the persistence from the contemporary *M* sequence, and for the same reason data would never return to the memorial system unchanged.

The studies of Penfield and Rasmussen (18, pp. 179–181) suggest that memory may be filed largely in the temporal areas. The report of Kennedy and Gottsdanker (10) indicates that the kappa electroencephalogram is maximal during tasks involving active recall. Since the kappa rhythm has a frequency of 8–13 per sec. and appears to be of temporal origin, it is possible that there may be some kind of interrelation between memorial and transient functions whereby the memorial "files" are scanned, at *M*f, in what subjectively can be described as the search for a given memory.

Among the major problems of learning are these: How are separate thoughts, ideas, or perceptions linked together? How is this influenced by motivating conditions? How do concepts develop? Considering these problems in a tentative and conjectural manner, how might they be handled within the context of the interacting *M*'s model?

First, the units which would be linked together might be the *M*'s. Second, the means of linkage might be the persistence of one *M* into the next—the greater the degree of commonality between *M*'s (in learning, these would typically be successive *M*'s) the stronger the linkage.

The learning of concepts would have to be based in some manner upon the linkages, i.e., the commonalities themselves. It is as if there is some means whereby those features which sequences of *M*'s tend to have in common are filed separately, or in addition to, the *M*'s per se—and eventually can themselves become *M* manifold input. This will be recognized as very similar to James' (9, p. 506) law of dissociation by varying concomitants. The essential meaning of it here is that concepts develop from the linkages in learning, or, to put it in another way, that it is chiefly concepts which link separate perceptions together.

To bring motivation into learning in a systematic way in the present model it is necessary to postulate that degree of linkages, i.e., *M*f and/or *M*p, are influenced by motivational conditions. There is really no evidence that this is the case, but our present purpose is simply to show that it could be the case, and there are some interesting data which may be considered in this light. First, there is the frequent appearance of delta rhythms during certain emotional states.[9] This could be interpreted to suggest that fixation in learning should be less during highly emotional states. The same type of interpretation could be used to explain why dreams are so poorly remembered. There is some evidence that anxiety increases the alpha frequency, or at least makes its fast components more dominant. This would imply that linkages during anxiety should be strong. It does *not* necessarily imply that behavior learned during anxiety would tend to be voluntarily repeated, or that it would be easily recalled—only that it would be well learned. Further,

[9] The emotion itself would, of course, be represented within the *M* pattern.

there are the interesting reports by Walter (25, 26) concerning the theta rhythm (frequency 6–8 per sec.). He finds a tendency for it to appear (chiefly in the temporal area) in children with various emotions and in adults with the cessation of a pleasurable stimulus. Walter conjectures that it may be a response to "minus-pleasure." Apparently it could also be described as a pattern which may appear with frustration, as a means, perhaps, of decreasing the degree of linkage of percepts which subjectively are felt to be frustrating.

This discussion of learning and electroencephalographic findings should be interpreted as extremely tentative and speculative. The purpose has not been to prove a point of view, but simply to show that the present model could be elaborated in such a way as to include conscious learning.

CONSCIOUSNESS

The word *consciousness* is used here to refer not only to the capacity for selective reactivity but also to include awareness. Thus man not only has experiences, but he is aware of having experiences; he not only knows, but he knows that he knows; he not only thinks, but thinking itself can become the object of thought.[10]

[10] This conception is currently out of fashion, perhaps because it seems too metaphysical. But it is a problem nonetheless. When the final understanding of the neural basis of consciousness is obtained it is unlikely that it will permit a real comprehension of just *why* that neural basis underlies consciousness. We must not expect the impossible. Such concepts as the curvature of space, a finite universe, or *n*-dimensional space cannot really be directly comprehended, but they may be useful nonetheless, because they give order and meaningfulness to the otherwise obscure. It is in this sense that we may seek the neural basis of consciousness. And when some day the solution has been found, the question of precisely *why* does this yet-to-be-understood neural system result in consciousness may be as ever-unanswerable a question as that of the child who asks, "*Why* does the earth pull the apple toward it?" What we may properly attempt, then, is to design a model with such characteristics as seem to be true of consciousness.

Viewed in this way, the stream of consciousness is more than just a sequence of separate, independent percepts. Rather, part of the immediately past thought becomes a part of the present thought. The just past thought becomes an object of perception in the present thought, which is itself a possible object for the succeeding thought. There is continuity to the stream of consciousness, each thought giving way to the next, yet maintaining something of the previous thought. Whether this phenomenal continuity involves simply the types of interaction referred to earlier, or whether it is based upon still more obscure mechanisms, it does seem to occur.[11] In terms of the present model the extension of one "thought" into the next, by whatever mechanism, would be in terms of *M*'s, and at least two of the pertinent variables would be *M*f and *M*p.

Directly, the greater *M*f and/or *M*p, the greater the degree of consciousness and vice versa. When the succeeding *M*'s did not overlap at all, there would be no consciousness at all, and no thinking in the usual meaning of the word. This would be the case, e.g., in deep sleep or deep anesthesia. It is noteworthy that under such conditions very slow brain waves do indeed appear, though it is, of course, merely a guess that they are indicative of *M*f.

When *M*f and *M*p are such that succeeding *M*'s overlap slightly, a vague and indistinct type of consciousness should be the subjective counterpart. It would be true consciousness, however, because there could now be interaction

[11] My point of view here is based very largely upon James' discussion of the Pure Ego. Readers who wish a more elaborate and enlightening presentation are advised to reread his *Principles* (9, ch. 10).

of succeeding "thoughts," i.e., there would be more than the mere refiring of previously filed memory units, unaltered in any way by the process. Rather, there would be an interplay of succeeding thoughts in which the thoughts themselves could be objects of further thought. Such a state should be characterized by poor memory for the sequence of thought and a wide range of association. It might be the situation during dreams. If *M*f and *M*p are low, but not markedly so, then the state of consciousness should be more like that during drowsiness, mild toxic confusional states, anoxia, or alcoholic intoxication. Learning, discrimination, and memory should be mildly impaired. As *M*f and *M*p increase, so should the degree of consciousness. When these variables are high there should be marked homogeneity of thought sequences and pronounced fixation in learning.

SUMMARY

A speculative theory of thinking has been presented in the form of a hypothetico-neurological model. It consists essentially of the view that the stream of thought may fruitfully be conceptualized as being based upon successive neural discharge patterns. These patterns have been termed *moments* (*M*'s). This paper has examined some of the possible effects of postulated interactions within an *M* sequence with regard to the problems of similarity, voluntary action, memory, learning, and consciousness.

What is needed now are straightforward experiments crucial to the major hypotheses. At the present time a study of the perception of high-frequency meaningful stimuli is under way. It is hoped that this will yield data pertinent to the present conceptualization.

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CLASSICAL CONDITIONING AND HUMAN WATCH-KEEPING[1]

D. E. BROADBENT

Applied Psychology Research Unit, Cambridge, England

It is the main thesis of the present paper that classical conditioning derives its chief interest and importance from the fact that it reduces learning to a much less central position than any other form of animal experiment. Paradoxical though such a position may seem, it will be shown that it enables us to include in a single category experimental results drawn both from conditioning and from work on human performance, the latter in situations normally regarded as extremely remote from the work of Pavlov. It is not, of course, intended to argue that learning does not appear in conditioning or even that Pavlovian types of experiment may not reveal useful information about the circumstances under which it will take place. But many of the facts of conditioning are not necessarily related to learning, and, insofar as existing theories of learning attempt to include them, they may well be engaging in an impossible task. The following remarks will not add still another theory of learning to the existing array, but rather try to clear the ground for future theorizing in this area of behavior.

Historically, psychologists have tended to regard conditioning primarily as a case of learning. Yet Pavlov himself regarded his own work as intimately related to the investigation of spinal reflexes by Sherrington (28, p. 378). Such a view gave importance not only to learning but also to further relations between learned responses once acquired; there was no learning among the Sherringtonian properties of the reflex, but there was an abundance of such phenomena as spatial and temporal summation, inhibition of one reflex by another, reciprocal innervation, and similar transitory rather than permanent alterations in response. Equally Pavlov sought to establish principles which would favor or impede the appearance of responses on particular occasions.

The physiological approach, however, caused Pavlov to assume that all stimuli falling on the receptor field produce some tendency for a response to appear, so that interaction between different responses takes place at the final common path as in spinal reflexes. Such an assumption may not be valid, and it is worth considering the consequences of denying it. It can scarcely be claimed that the individual ideas set forth in what follows are original; besides those which are given specific references, the general principles asserted are to be found in a different language in a celebrated chapter by William James (19, ch. XI)—a chapter which opens with a denunciation of the emphasis placed by his comtemporaries on learning alone! Some degree of novelty, however, may be found in the application of these principles to classical conditioning.

A Fresh Analysis of the Pavlovian Situation

There is some mathematical evidence to suggest that even the human nervous system would be incapable, through limitations of size, of simultaneously analyzing all the information received by its sense organs (16). It is not unreasonable, then, to suppose, that only a

[1] This paper was written while the author was receiving a grant from the British Medical Research Council. What is good in it is probably due to Professor Sir Frederic Bartlett, F.R.S., and Dr. N. H. Mackworth.

portion of this information is analyzed at one time, that is, that only one part of the total stimulation present is capable of initiating complex responses at a given instant. This suggestion has been linked by Hick to the subjective concept of "attention" (16). The possibility is further favored by the long succession of experiments on perceptual set in human beings, some of which have shown that adequate response to one part of the stimulus situation is incompatible with adequate response to another part. This has recently been demonstrated for the auditory field, thus eliminating questions of foveal and peripheral sensitivity (11). Such incompatibility may perhaps appear only in adults and be developed by learning from an original unselected reception of information, but it should be remembered that Pavlovian conditioning is normally carried out on adult mammals of reasonably high complexity. A process of selection among the various stimuli presented is, therefore, not unlikely to appear in such experiments.

There is some evidence, of which the references to be cited are representative rather than exhaustive, that certain properties of a stimulus will make it more likely to be selected from among its competitors, and to retain its dominance if selected by chance. *Physical intensity* is one such property (2, 12). Another is position of the general class of similar stimuli in a hierarchy which is independent of physical intensity but may be established by experiment (e.g., stimulation of a pain receptor might have high priority, and so might food for an animal which has been deprived of it). This property may be called for brevity *biological importance*, and as the above examples show, it may be established innately or by learning, and may be permanent or dependent on the temporary state of the organism (21, 34). A third property of the stimulus is likely to be important for selection, unless the organism is to be dominated continuously by one stimulus; this is degree of difference from the preceding or more remotely past selected stimulus, that is, *novelty* (1, 4). There is no need to assume that the selected aspects of the situation should produce any overt response, but if a stimulus is to produce a response it must first be selected. Appearance of a response which is known to depend on a certain stimulus may then inform us that this stimulus is dominant, but absence of response may indicate merely that the dominant stimulus has no dependent response. In this case, as will be seen, responses to subsequent and simultaneous stimulation may be affected.

Such principles do not involve learning in the sense of a permanent change in the response produced by a particular stimulus; yet they are capable of accounting for many of the features of conditioning.

(*a*) If the stimulus which an experimenter applies is to produce any response, innate or acquired, it must possess marked advantages over all competitors: The animal's surroundings must therefore remain unchanged for a considerable time before the experiment, contain no important objects such as humans, and not contain stimuli of high physical intensity (28, p. 20).

(*b*) If a stimulus is repeatedly applied, as its novelty becomes less it is less likely to be selected and so to produce any response. A time interval after such a repeated application will permit selection of other stimuli and so, by the third of the above principles, once more give a preference to the original stimulus. (The familiar phenomenon of extinction and spontaneous recovery. It will be noted that this is a theory of extinction by "competing stimulus." Its advantages over existing theories will be discussed later.)

(*c*) If between two presentations of the original stimulus there is inserted a different stimulus having a high priority according to these principles, the latter will be selected; when the original stimulus is next presented, it will then be more markedly different from the preceding selected stimulus, and so by the third principle will be more likely to produce a response. (Disinhibition, and also the retarding of temporary extinction by continued reinforcement.)

(*d*) The foregoing result will naturally apply only if there is a moderate interval between the two stimuli. If the fresh high priority stimulus is presented simultaneously with, or very closely previous to, the original stimulus, it will markedly lower the probability of the latter being selected. (External inhibition, and the difficulty of backward conditioning.)

(*e*) If a series of presentations of the original stimulus is followed by the presentation of a different and moderately high priority stimulus to which the acquired response is inaction, this will have an effect similar to that of disinhibition. ("Positive induction" [28, p. 189]. This is a case where a stimulus having no response has a subsequent effect.)

(*f*) This induction effect will presumably vary with time since the presentation of the intruding stimulus and with the similarity of the two stimuli. ("Irradiation" [28, pp. 153 f.]. It is not necessary to suppose that the function takes the particular form stated by Pavlov, but only that both variables enter into it [22].)

(*g*) The presentation of a reinforcing stimulus only after every other or by every third presentation of the conditioned stimulus may enhance its effect by preventing reinforcement from losing its "novelty" priority. ("Partial reinforcement" [28, p. 334]. This effect will naturally be most marked when trials are massed [20].)

(*h*) Extending the time of presentation of a stimulus is likely to change the relative priorities of different stimuli, a formerly high priority stimulus becoming low and vice versa. ("Paradoxical phase" [28, pp. 379 f.].)

(*i*) Similar effects will appear not only with learned responses but also with unlearned ones: thus the lid response to moderate sounds will disappear with repeated presentation and reappear when a different stimulus is inserted between presentations. Increase in the intensity of the stimulus or use of a stimulus of greater biological importance makes such a disappearance unlikely (27).

Some comments may be made on these points. It will be noted that the two principal novel concepts are the classing of reinforcement with disinhibition and the use of what may be called a "competing stimulus" theory of extinction as opposed to the existing theories of competing response or of depression of activity (17, pp. 115 f.).

As regards the role of reinforcement, it should be emphasized once again that it is not intended to advance a theory of learning. Rewards may or may not be necessary to produce a change in the response produced by a given stimulus; all that is being argued is that after learning has taken place the stimulus may be made to secure repeated selection by interspersing presentations with other stimuli of high priority—and that in this respect novel or intense stimuli should be classed together with those of biological importance. Such a view has advantages in considering the factors which maintain an established type of response in human beings and also in some experiments showing that "punishing" stimuli as well as "rewarding" ones (26) may assist performance. Further, it is supported by the work of Pavlov

(28, p. 390) showing that reinforcement of an extinguished response has an effect as transitory as disinhibition. Contiguity theorists may perhaps feel that the present approach offers a promising line of assault on existing reinforcement theories, particularly when recent work on reinforcement without drive reduction is considered (32, 33), but they will be going beyond the evidence in doing so.

The question of the general relationship of this approach to learning theory also arises in regard to the "continuity" controversy: Is all stimulation presented to an organism equally likely to form learned connections to responses? This question again lies outside the present scope; it is generally admitted that in the adult, selective response does appear, but it may be true to say that it is a consequence of earlier learning or that learning of other stimuli may be going on though undetected. The present argument is, however, that the facts of extinction follow from those of selective response. Consequently, it seems illogical to neglect the latter in learning theory and yet to include the former. If either is to be included, it should be selective response since it is logically prior. It should also be noted that existing tests of continuity are inadequate: they only contrast unselected learning with completely selective learning. It is also possible (using the language of the present theory) that only selected stimuli are learned, but that under favorable circumstances the selector mechanism will "scan" different stimuli in succession. Spaced learning might, on such a view, favor continuity theories, while massing would oppose them: this is in fact the case (6). But existing data are insufficient to shed light on this point.

We cannot leave the question of learning theory without noting the capable and painstaking suggestions of Berlyne (1, 2, 3, 4) to link phenomena of the sort considered here with the Hullian system. His emphasis on an internal "perceptual response" and his classing of novelty as a "drive" appear to be similar in many ways to the present view, except that he applies the same Hullian postulates both to the external responses for which they were first stated and to his internal one, whereas the writer would suspect that different principles will appear in the two cases and that the system of Hull confuses the two. Most probably this divergence reflects a general difference of opinion on the relative value of theories which cover all behavior but which require numerous simultaneous assumptions and of specific theories whose scope is limited but whose ratio of assumptions to observables is rather lower. On this point the writer's view of scientific method differs very considerably from that of Hull's supporters. It is interesting to note that Hull himself mentioned some of the possibilities suggested here, in particular to explain the difficulty of delayed conditioning (18, pp. 207 f.), but apparently did not realize that they were alternative, rather than additional, to an orthodox extinction theory.

As for the advantages of a theory of extinction by competing stimulus, it will be seen that it avoids the weaknesses of both the existing theories. On the one hand, it would be expected that extinction would be favored by massing rather than spacing while acquisition would reverse this relation, and that correlations between rates of acquisition and extinction would not be positive. These points are usually regarded as favoring extinction by depression of activity; but the detectable presence of responses to competing stimuli and the hastening of extinction when these responses are encouraged tells against this last view while being perfectly consistent with a theory of extinction by competing stimulus. It is, of course, only tempo-

rary extinction that is covered by this theory: permanent extinction is a problem of learning to refrain from response and so falls into the field of learning theory. A two-factor theory of inhibition is already current (18).

The explanation of Pavlov's less central concepts must be regarded as tentative, since accounts of the experiments seem to indicate that the name "paradoxical phase," for example, was used for several states arising from different procedures and possibly different in nature. From the present standpoint, it would seem that Pavlov's terminology was led astray by his belief that interference between different stimulus-response links took place at the level of the motor pathway. He could not, for example, assimilate positive induction to disinhibition because the latter required, for Pavlov, an active response to inhibit previous inhibition, and the negative inducing stimulus had no such active response (28, p. 191). The illustrations given show, however, that the more exotic features of Pavlovian conditioning are open to reasonable explanation from the present viewpoint and deserve a more thorough experimental investigation than they have usually received. We reach here the central peculiarity of classical conditioning—that the dominance of the situation by the experimentally controllable stimulus makes it possible for us to follow the movements of the animal's attention, using the learned response as a detecting instrument. The principles of attention are no doubt operative in all behavior, including instrumental conditioning and other laboratory situations, but they are always confused with learning and with other factors in a manner hardly resolvable except by instructing the animal on the requirements of the task—and that can be done only with one species. As soon as emitted responses are involved, moreover, we face the problem of the relation between the perceptual mechanism and that which controls such emitted responses, for it is not clear how far the two are independent, or, in common speech, how much we must attend to what we do (11). In such a field our problems must be separated for experiment, so far as that is possible without artificiality, and for the study of the processes which select now one part of the environment for response, now another, Pavlov's technique has no rival in infrahuman animals. His goal was to introduce order into the apparently lawless behavior of organisms, which, faced with the same situation, do not always react in the same way. His analysis of his results (and especially his speculative physiology) may have been imperfect, but his experiments were far more subtle than his successors have usually believed.

PAVLOV AND APPLIED PSYCHOLOGY

The most critical of the deductions made in the previous section is probably the last. Sharp lines divide most theories of learning from reflex behavior on the one hand and from the performance of the human worker on the other, lines bridged only by a belief in the unity of all behavior. Reference has already been made, however, to the data given by Oldfield (27) on the lid responses to sound; similar observations given in the same paper are equally understandable on the present view. We may perhaps note here in particular, as they have been criticized, the results drawn from Rawdon-Smith (29), who found that the auditory threshold was elevated after exposure to a high intensity tone, that this applied to the opposite ear as well as that exposed, and that the elevation was temporarily abolished by a novel visual stimulus. Some of these results were not borne out by later workers (e.g., Caussé and Chavasse [13]). Rawdon-Smith, however, gave

his tests (to an *S* in a featureless room) for a prolonged period. His stimulating tone lasted two to four minutes while most of the stimuli employed by Caussé and Chavasse were of half a minute or less, and the period of threshold testing continued for some time after exposure —more than five minutes in the experiments on "disinhibition." The later workers used a shorter testing technique. It would be expected on the present view that such prolonged testing would cause some elevation of the auditory threshold even if no high intensity tone was applied during the series and that elevation due to this cause would be abolished by a novel stimulus. This is a normal hazard in audiometry; the writer has often observed similar elevations while giving lengthy audiograms to naive *S*s, making it necessary to introduce variations in the procedure if the lowest possible threshold is to be obtained for each *S*. Again, the operating instructions for one commercial audiometer include the following statement: "Do not keep the tone interrupted for long periods, as the patient will tire, due to the strain of listening: conversely do not keep the tone on for excessive periods—not only may the patient's attention wander, but also he may tire of holding in the signalling button" (24, p. 7). Those who fail to find elevation of the threshold on the unstimulated ear may, then, have avoided this danger as a normal point of technique, and Rawdon-Smith's results can be explained on the present view.

This explanation is more satisfactory than that given by Caussé and Chavasse themselves, who feel that Rawdon-Smith may have been detecting an elevation due to sound leaking from one ear round to the other. But Rawdon-Smith had failed to detect any elevation *using his testing technique* when applying a stimulating tone of the intensity to be expected from leakage. The work of Caussé and Chavasse is highly important simply because it does eliminate central factors of this general, not peculiarly auditory, sort, and so sheds more light on peripheral auditory fatigue and thus on theory of hearing: but this does not mean that the central factors are unreal.

The most direct analogy to Pavlovian conditioning, however, lies in tasks where the human *S* is required to make a voluntary response whenever an infrequent signal occurs. We would expect that, despite the almost immediate learning displayed in such a situation, the characteristic features of extinction would appear. The first statement of this parallelism was made by Mackworth (23) for performance on the Clock Test, a task similar to radar watch-keeping.

It appeared that:

1. Responses to the infrequent stimulus became much less common after a period of continuous watch. (Extinction.)

2. A short interval between periods of testing temporarily restored full efficiency. (Spontaneous recovery.)

3. A novel stimulus (a telephone message) temporarily restored full efficiency. (Disinhibition.)

4. The following of every stimulus by an auditory message informing *S* of his performance prevented deterioration. (Continued reinforcement.)

To these similarities others have been added by Broadbent (7), using a dial-watching task.

5. When signals come from several possible sources, some of these sources are naturally more likely to receive a response than others, but when the time of monotonous watch is extended these priorities are reversed and the initially neglected sources become favored. (Paradoxical phase.)

6. On the individual *S*s, there is a correlation between degree of decrement in performance during each watch and amount of recovery in the interval between watches. The extremes of this

correlation correspond to Pavlov's description of excitable and inhibitable types (28, p. 285).

7. The signal itself introduces a novelty into the situation, and the occurrence of each signal restores performance partially to that appearing at the beginning of the watch. (Disinhibition by a novel positive stimulus [28, p. 62].) This finding allows us to note a further point in the work of Mackworth (23), namely that *S*s who respond to more signals maintain performance better.

We also note that:

8. On the Clock Test and radar test the effect of intervals between signals was complicated by "expectancy" effects, the intervals being shorter than those used by Broadbent. One of these effects was that a rapid series of signals actually resulted in a lowered probability of response, since, subjectively speaking, the *S* expected long intervals between signals and so relaxed his attention after each. This is comparable to one of the Pavlovian demonstrations of "negative induction" (28, p. 202).

There seem, therefore, to be a number of observational analogies between the conditioning situation and that of human watch-keeping, and the later work has supported Mackworth in suggesting these analogies. Such a similarity makes an identity of underlying mechanism very probable. The present writer departs from the original discoverer of the parallelism, however, in interpreting conditioning in the light of attention rather than interpreting vigilance experiments in the language of conditioning. Certain results may be noted from experiments on human workers in other situations which, while not directly comparable to conditioning, confirm the general validity of the principles stated.

1. If a task is set to an *S* and his responses to it are allowed to appear at their natural rate, the task will after a while cease to be novel and so stimuli from the remainder of the environment will tend temporarily to weaken its dominance over response. Any such weakening will immediately render the task more novel, leading to its resumption, and so performance will not so much deteriorate as become irregular in time (5, 8). Removing the timing of the task from *S*'s control will cause an actual decrement in output with continued performance (8). (The relation between this situation and the watch-keeping one is considered in greater detail in the latter paper.)

2. The presence of an intense but irrelevant stimulus, such as noise, as well as the task to be performed, will initially have little effect on the task. There may be a very transient decrement due to an "external inhibition" effect, but this will pass (14). Indeed it may be succeeded by actually improved performance by the "disinhibition" mechanism (25, 31). As the task is continued for prolonged periods, however, the strong competing stimulus will accentuate any tendency to show a decrement (8, 9). Serious effects of noise will therefore only appear on prolonged tasks and especially on those which require continuous reception of information from a particular part of the surroundings.

3. It has been pointed out by Gagné (15) that a number of studies carried out at the USAF Perceptual and Motor Skills Research Laboratory on the learning of motor skills under massed and distributed practice are difficult to interpret on the basis of the classical "inhibition" view, but rather suggest the importance of variations in the stimulus complex.

It will be remembered, then, that originally we stated three principles as governing the selection of a stimulus for further analysis and response from among those present. These principles

account adequately for a great part of the data from conditioning and for some of that on continuous human performance where learning is reduced considerably. They are not principles in the sense of postulated states within the organism, whether excitatory, inhibitory, satisfactory, or expectant, but rather in the sense of generalized rules about the properties of stimulation important for certain behavior to appear. Equally they are not concerned with permanent changes in behavior, but with transitory and conditional ones, and it will be felt a sufficient advance if they are accepted as such. A theory of learning is an ultimate aim for psychology as a whole, but we still have no adequate evidence to sustain one.

Finally, it is felt that this approach reduces linguistic questions to a proper subsidiary position. Some may prefer to speak of the extinction of responses by human *Ss* in vigilance tests, others of the boredom of Pavlov's dogs, and still others of a shift in the source of the information flowing through the receptor field. There are no doubt reasons for preferring each such language, but the important point is the similarity of the experimental relations described in them. Further experiments, too, are badly needed. As examples we may suggest:

(*a*) Attempts to condition responses to stimuli which have been presented many times while still indifferent. (The present theory would suggest that this would be very difficult, and that this might explain the difficulty of demonstrating sensory preconditioning [30].)

(*b*) Determinations of the optimum time interval between signals of different kinds and in various modalities, when such signals are used for a lifelike skilled task, or in animals, to elicit conditioned responses.

(*c*) Closer examination of the influence of background stimuli, both relevant and irrelevant, upon the execution of skills, and particularly of the effects of similarity between such stimuli and those central to the skill, and of the time relationships involved.

Such experiments must be welcomed whether they are described as dealing with induction and irradiation, with the influence of dissimilarity and sequence in time on attention, or with the corresponding informational analysis of the same problem.

SUMMARY

1. Evidence both from mathematical considerations and from experiment favors the view that only certain aspects of the total stimulus situation can initiate complex responses at one time, and stimuli possessing intensity, biological importance, and novelty are most likely to be selected at any time.

2. These principles enable us to account for many phenomena of classical conditioning, including some which are usually neglected, and give an explanation of extinction (in terms of competing stimuli rather than competing responses) which has advantages over existing theories.

3. They further provide a rational interpretation of certain experiments on human beings which are usually regarded as being of purely industrial importance; for example, the pacing of factory work by the speed of the machine may prevent occasional shifts in selection and so by the novelty principle increase apparent fatigue.

4. They do not include an account of learning, as a permanent change in response to a particular stimulus, but are intended to remove certain confusing phenomena from the field to make way for future theories of learning. It is suggested that Pavlovian conditioning is of importance as making an account of these phenomena possible and that further experiment on such matters as

Pavlovian "induction" is highly desirable.

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THEORY CONSTRUCTION FOR BLODGETT'S LATENT LEARNING

JOSEPH WOLPE
University of the Witwatersrand, Johannesburg

Comments on the Hullian Approach

When a Hullian insists that his theoretical constructs shall be derived *only* from observations of stimulus and response he handicaps himself in two ways. (*a*) He cuts himself off from helpful neurological information; for when behavioral facts can be related to each other in several theoretically possible ways the neurological facts may point to the exclusion of certain of the possibilities (21, 22, 23, 24, 25). For instance, neurological facts were shown (25) to contradict Spence's prediction of the character of the gradient of generalization of extinction effects (15), when on the available behavioral facts alone that prediction was just as plausible as any other. (*b*) He loses sight of the fact that the events that intervene between stimulus and response take place *inside the organism*, and that consequently behavioral antecedents may have intraorganismal consequents *without behavioral consequents*.

It is their failure to realize the latter possibility that has made latent learning such a stumbling block to Hullian theorists. Latent learning provides no behavioral consequent to which a Hullian construct can be anchored. Therefore, committed to the assumption that there can be an all-inclusive theory of behavior in terms of their characteristic constructs, the Hullians have no real answer when their inability to account for latent learning is pointed out by such critics as Hilgard (5), Thistlethwaite (16), and Birch & Bitterman (1). Consequently, some Hullian theorists have been understandably anxious to explain away latent learning. For instance, Miller (10) has taken the view that latent learning is merely a case of very weak learning. Nobody denies that a weak measure of manifest learning accompanies latent learning, but what has to be explained is the remarkable improvement in manifest learning immediately after the introduction of food reward in the experiments of Blodgett (2) and Tolman and Honzik (19). Maltzman (8) has ingeniously tried to explain the improvement on the basis that during the unrewarded trials there is, through a process of habituation, a gradual weakening of responses in competition with the response finally to be rewarded. But, as Thistlethwaite (17) has pointed out, Herb's observations (4) contradict this proposition, and in any case experimental experience in general does not seem to show that habituation to apparatus accelerates conditioning to such an inordinate extent.

Comments on the Tolman Approach

The critics of the Hullian position have, by contrast, been quite happy about latent learning, explaining it by hypotheses of the Tolman pattern. In theorizing thus their advantage is that they are not merely correlating molar facts of behavior, but are also, in a way, *looking inside the organism*. The inner events they postulate are "expectancies" and "cognitive maps." Their explanation of the dramatic improvement in performance following the introduction of food in the Blodgett type of experiment is this: The feeding changes the animal's goal-box expectancy so that on his next run his hunger drive motivates him to use his "cognitive map" of the

maze in such a way that he takes the path to the goal box rather than other paths.

Although in this explanation such words as "expectancy" are objectively[1] defined (18) the definitions have a looseness that seems to stem from the mentalistic tradition of the words. We shall examine below whether the explanation can still be maintained if what Tolman means by "expectancy" is expressed in more rigorous terms.

It may be said that an expectancy is formed when the stimuli S_1 and S_2 have been presented to an organism in temporal contiguity a number of times, and it is subsequently found that presentation of S_1 alone causes a cognition of S_2 to be evoked in the organism. The expectancy is, then, the evocation by S_1 of the cognition of S_2.

Now let us examine more closely the mechanism by which this expectancy is evoked. It is self-evident that this mechanism must consist of internal organismal *responses* to S_1. When, for example, in an experiment on sensory preconditioning (e.g., 3) the repeated co-occurrence of stimuli S_1 and S_2 enables S_1 alone to evoke responses characteristic of S_2, these responses could well include such responses as would give the organism a *cognition* of S_2—this, by definition, constituting an expectancy. But as this expectancy would merely be a consequent of some of the S_2-connected responses it could certainly not be invoked to explain how S_1 came to be able to produce the S_2-connected constellation of responses. Expectancy cannot be part of the mechanism of the performance of which it is a product.

Now if, in the Blodgett type of experiment, S_1 stands for choice-point stimuli and S_2 for goal-box stimuli, unrewarded runs will enable S_1 to evoke S_2-connected responses. With the first introduction of food into the goal box a new set of responses (characteristic of feeding) will be conditioned to S_2, so that at the next run a changed pattern of S_2-connected responses will be evoked by S_1. The expectancy-producing responses may be assumed to share in this change, but, as argued above, cannot be part of the causal chain determining the change in the *immediate responses* to S_1.

But may not S_2-connected responses be intermediaries in the greatly increased tendency for S_1 to evoke the appropriate turn? Seward (13, 14) has an ingenious theory that has potentialities for answering this question, and has promised to show how it "takes care of the phenomena of latent learning and of sudden changes in amount of rewards" (13, p. 372). A central idea of Seward's theory is that once a right turn at the choice point has been followed by reward in the goal box, at the next arrival at the choice point a kind of secondary drive ("tertiary motivation") produced by the proprioceptive cues from *starting* to turn right (VTE) preferentially facilitates right-turning (14). It seems precarious to lean so heavily on proprioceptive cues in view of the unimpaired maze performance of animals in which gross lesions have grossly altered proprioceptive impulse patterns (6, 7).

A NEUROPHYSIOLOGICAL VIEW OF LATENT LEARNING

It is rational to turn for an explanation of latent learning to the place where it occurs—the nervous system. Thirteen years of elaborate discussions about

[1] The way Tolman defines expectancy is not objective if objectivity in a definition implies that its referent is at least potentially observable by the observer and therefore part of the causal continuum from the observer's point of view. The cognitions of the subject can never be observed. Only the neural processes that are the correlates of these cognitions have the potentiality of filling the gap in the observer's causal continuum.

theory construction have left as firmly grounded as ever Pratt's assertion: "Only physiology can afford an explanatory frame of reference for the facts of psychology" (11, p. 131).

The hypothesis of latent learning to be outlined here is an extension of the neurophysiological hypothesis of learning previously advanced (22, 24). According to this hypothesis learning depends upon the development of functional connections (i.e., synapse formation) between neurons in anatomical apposition. On a basis of experimental evidence certain relationships were deduced which will be set down here in the form of postulates.

Postulate 1: The relevant presynaptic neuron must deliver impulses to the synaptic zone during, or just before, firing of the postsynaptic neuron. This provides sensitizing preconditions which we shall call *reinforcement-sensitivity.*

Postulate 2: Drive reduction must follow the formation of reinforcement-sensitivity if the process of synapse formation is to be completed. (Drive reduction means reduction in the number of impulses arriving at this synaptic zone from all sources. All stimuli are possible sources of impulses, and the number of impulses varies directly with strength of the stimuli. The reduction in number of impulses following the cessation of a weak visual stimulus would obviously be much less than would follow from marked relief of hunger.)

Postulate 3: Amount of synapse formation (and therefore amount of learning) varies directly with amount of drive reduction, other things being equal.

Postulate 4: The shorter the interval between the formation of reinforcement-sensitivity and the ensuing drive reduction, the greater the amount of synapse formation. Reinforcement-sensitivity fades with time.

To the above an additional postulate is now added:

Postulate 5: Amount of synapse formation varies directly with amount of reinforcement-sensitivity upon which the drive reduction acts.

Let it be assumed that in a latent-learning experiment of the Blodgett type a rat is eventually to receive food in the right compartment of a T maze. Thus, the habit to be built up will be a right-turning (R_r) in response to the cues of the choice point (S_{cp}). With reference to Fig. 1 we shall first discuss the hypothetical neural events associated with an unrewarded run in which the animal does a right turn at the choice point, and then the consequences of introducing reward.

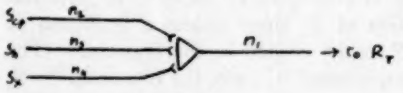

FIG. 1.

Let n_1 represent a neuron leading to the response of turning right (R_r). During an unrewarded run, while n_1 is firing, impulses are being delivered to it by n_2 (from choice-point stimulation S_{cp}), by n_3 (from hunger stimulation S_h), and by n_4 (representing miscellaneous stimulations S_x). For n_2 to deliver impulses while n_1 is discharging them means that sensitivity for reinforcing the $n_2 \cdot n_1$ connection is created, and the amount of reinforcement that finally results will depend on (*a*) how much reduction of drive follows in the whole synaptic zone and (*b*) how soon it follows. During this unrewarded run the only drive reductions to occur are those correlated with the cessation of action of weak stimuli such as S_{cp} and S_x. Thus reinforcement is weak, and manifest learning is weak.

To account for latent learning the following supplementary hypothesis is suggested. A run followed by an adequate food reward would lead to sufficient drive reduction to use up *all* the

reinforcement-sensitivity produced at $n_2 \cdot n_1$ during that run. On the other hand, the weak drive reduction following an unrewarded run *leaves a residuum of reinforcement-sensitivity.* Part of this residuum fades, and *part remains in a cumulable form* that is activated by, and summates with, the new reinforcement-sensitivity produced during the next run. After a number of unrewarded runs there will be a considerable accumulation of reinforcement-sensitivity at $n_2 \cdot n_1$, and now a food-rewarded run will strengthen inordinately the $n_2 \cdot n_1$ functional connection (see postulate 5 above).

Though based on the typical Blodgett experiment the above hypothesis is also in complete harmony with the variations thereof. *Whenever* reward at last closely follows a previously unrewarded stimulus-response sequence, reinforcement will be strikingly great, and it does not matter if the response is a turning into a blind (4), if the reward is given outside the maze (9), or if the unrewarded runs are variably interrupted (20). In the last-mentioned case relatively less striking postreward learning would be expected at the more advanced choice points where the animal would have passed fewer times.

Corollaries

The following corollaries emerge from the above hypothesis:

1. *The greater the number of preliminary unrewarded runs the greater will be the increment in learning following the first rewarded runs.*

Blodgett's own experiment (2) seems to confirm this. The mean errors of his 3-day-unrewarded group decreased from 2.7 to 0.3 after four rewarded runs, while those of his 7-day-unrewarded group decreased from 2.3 to 0.2 after two rewarded runs. The mean improvement per rewarded run was thus 0.6 for the 3-day group and 1.15 for the 7-day group. The curve of improvement following the first reward flattens out for the 3-day group while remaining very steep for the 7-day group. This suggests that the accumulated reinforcement-sensitivity was entirely absorbed in the course of the first big drive reduction in the case of the former group but not in the case of the latter. Presumably, the amount of drive reduction to follow one reward of the size given was insufficient to use up the larger amount of reinforcement-sensitivity accumulated by the 7-day group. This presumption could be tested by repeating Blodgett's experiment with varying amounts of reward for different groups.

2. *The less the manifest learning that follows the unrewarded runs the greater the increment of learning that will follow the first few rewarded runs.*

The difference between the results of Blodgett (2) and those of Reynolds (12) seems to support this corollary.

Two Problems:

Does cumulable reinforcement-sensitivity (latent learning) slowly fade with time, and do reciprocal-inhibition effects (25) weaken it?

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AN APPLICATION OF HELSON'S THEORY OF ADAPTATION LEVEL TO THE PROBLEM OF TRANSPOSITION [1]

HENRY JAMES
University College, London

One of the usual requirements of a theory of behavior, psychological or physiological, is that it shall give a step-by-step account of the successive coordinations and differentiations, both sensory-sensory and sensory-motor, which lift behavior above the level of the schematic reflex. If this criterion is applied to the Gestalt and S–R theories of stimulus equivalence, both are found wanting. The neurophysiological theory of Köhler (17), preoccupied perhaps with shifting the problem of "similarity" from the periphery to the central nervous system, gives no account of the events which link the motor system with the cerebral activity aroused by the stimulus. The same objection may be raised against the recent use of such psychological constructs as "differentiation" (28) and "mediating process" (21) to account for certain instances of equivalence behavior; until something is known of how the organism may at once be influenced by such general states *and* respond to a particular stimulus in a particular way on any one trial, this kind of model raises more problems than it solves. Hull's (12) theory of stimulus equivalence, on the other hand, is, in a sense, more precise concerning the train of events from receptor to muscle (provided one casts a blind eye on "afferent neural interaction"). We may doubt the usefulness, however, of arguing back, as acceptance of the theory would force us to, from an observation of similar responses to the existence of sets of mediating stimulus continua along which generalization may take place. The existence of a continuum is inferred from certain established relations between measurable differences in stimuli presented and responses observed. Such complex stimuli as geometrical forms and nonsense syllables cannot be scaled independently of *S*'s response to them, and hence the psychophysical relations necessary for the study of similarity within the framework of Hull's theory cannot be determined.

If we accept Razran's (24) recent review of the literature, the study of stimulus equivalence at the level of Pavlovian irradiation throws us back upon the same problems. He concludes that the *S* in a classical generalization experiment forms some sort of crude rating scale of similarity against which the new stimuli are assessed. We are thus, it seems, forced to assume that a central process, of whose properties we know little, intervenes between the receptor stimulus and the response, which leads us back to the question of how these "sets" determine a particular response on a particular trial.

Similar problems have arisen from the traditional treatments of visual space perception; Gestalt theory again ignores the question of response, and functionalism provides no account of the way in which the various cues are integrated to produce a stable world. A way out of this dilemma is indicated by Gibson's recent suggestion that the problem of integration should be kept at the periphery. "If," he writes, "the total stimulation contains all that is needed to account for visual perception, the hypothesis of sensory integration is un-

[1] I am grateful to Messrs. A. Summerfield and A. Jonckheere for their helpful criticisms of this paper in manuscript.

necessary" (6, p. 25). While this type of analysis has certain limitations, its virtue is that it no longer requires us to map out the train of events which intervene between the receptor stimulus and the end response; rather it demands a psychophysical model in the form of sets of quantitative statements of relations between measurable properties of stimuli and some convenient metric of response frequency or extent. Ideally, the parameters of the mathematical model do not require, although they permit, translation into psychological or physiological constructs; but the limitations which the psychophysical approach places upon the range of appropriate stimulus situations, together with the absence of a purely rational basis for deriving the necessary equations, make this ideal scarcely realizable. Rather, as Graham points out, "the application of psychophysical method is a first step in setting up the conditions that allow for a scientific description of a stimulus-response relation. Properly conceived, it specifies variables. It also poses a relation between variables requiring explanation" (7, p. 117).

Psychophysics, in the sense in which Gibson uses the term, has not found much favor with students of learning, although Pratt's (23) paper on the "Law of Disuse" gave a clear indication of its usefulness in this field. In the following pages we shall attempt to show that by suitably adapting the method to the needs of the learning situation, it is possible to avoid the sort of problem outlined above, which arises when the more conventional hypothetical-construct–intervening-variable type of model forms the basis of our description of the mechanisms of stimulus equivalence.

ADAPTATION LEVEL AND TRANSPOSITION

The transposition experiment consists essentially in training S to respond differentially to two stimuli, X and X', the critical characteristic of which lies along a single physical continuum. The S is then presented with two new stimuli, one of which may be X or X', and rewarded for whichever choice he makes. The problem is to determine whether he has learned to respond to the absolute properties of the training stimuli or to the relation between them. The main facts which have been established in this sort of experiment are as follows:

1. Within a certain range of stimulus variation, transposition tends to occur (i.e., the test response is to the relative rather than to the absolute values).
2. Transposition tends to decrease as the distance of the test from the training stimuli increases.
3. Transposition tends to improve as testing is continued, even though S is rewarded for whichever response he makes.

These facts, suitably expressed in terms of the stimuli used and the frequency of the responses observed, constitute the main psychophysical relations for which any theory of transposition has to account. In terms of the present approach, the problem is to give them a compendious quantitative expression, and to this end we shall consider Helson's theory of adaptation level (9, 10, 11). The reader is referred to the original articles for a detailed exposition of the theory. In essence it assumes that:

> . . . effects of stimulation form a spatio-temporal configuration in which order prevails. For every excitation-response configuration there is assumed a stimulus which represents the pooled effect of all the stimuli [with which the S is or has been presented] and to which the organism may be said to be attuned or adapted. Stimuli near this value fail to elicit any response from the organism or bring forth such neutral responses as indifferent, neutral, doubtful, equal or the like, depending on the context of stimulation. Such stimuli are said to be at adaptation level. Conversely, adaptation level may be quantita-

tively specified by giving a value of stimulus eliciting the neutral response.

Stimuli above the adaptation level (AL) are assumed to establish positive gradients with responses of one kind, and stimuli below establish negative gradients with responses of an opposite kind. Positive or negative enhancement occurs as stimuli are above or below the AL; which may be high, medium or low, depending upon a number of factors in the stimulus configuration like background in vision, comparison stimulus in psychophysical judgments and in the organism which may be "set" at a given level due to effects of previous stimulation. A stimulus far above the stimulus range may raise the AL so high that most of the stimuli will be below the AL and the majority of judgments will be of the "negative" sort, while the opposite will be the case with a stimulus far below the range (10, p. 2).

In applying this hypothesis to transposition behavior we make the following additional assumptions:

1. In the process of training a response, say to the brighter of two stimuli, there is established in *S* a neutral point or region, corresponding to the AL of Helson.

2. The *S* learns to avoid stimuli which are less intense than the neutral point and to approach those which lie above it.

3. The process of establishing a neutral point is independent of reward and punishment. While the latter may determine the character of the responses which stimuli on either side of the neutral point will evoke, they are assumed not to contribute to the location of the neutral point itself.

4. New stimuli presented to *S* after training will induce a gradual change in the position of the neutral point. The rate of shift probably depends on such factors as number of trials with the training stimuli (13), distribution of practice, intelligence, etc.

From these assumptions it follows that if the test stimuli fall one on either side of the training AL, perfect transfer will be predicted. If both fall on the same side of the training AL, then responses will be random with respect to the critical cue until continued familiarity with the test stimuli has shifted the AL in the appropriate direction. In checking this point of view against some of the published literature (1, 3, 15, 25), we have assumed that the test stimuli have an immediate effect upon the location of the neutral point; that is, by analogy with the case of psychophysical judgment, we have assumed that the training neutral point has the function of a standard which initially "anchors" the transposition responses.

The formula found adequate by Helson for predicting the AL in weight-lifting and constancy experiments was a weighted geometric mean of the background (standard) and sample (series) stimuli. To test the hypothesis against rat data from Kendler (15) and Ehrenfreund (3) we have used the equation

$$\text{Log AL} = \tfrac{1}{2}\left(\frac{\Sigma \log X}{n} + \log C\right) \quad (1)$$

where AL is the neutral point for the initial test situation, X is the test stimuli, n their number, and C is the training neutral point (estimated as the geometric mean of the training stimuli). For Spence's (25) data on transposition in chimpanzees,

$$\text{Log AL} = \left(\frac{\Sigma \log X + \log C}{n+1}\right) \quad (2)$$

and for Alberts and Ehrenfreund's (1) experiment with preverbal children,

$$\text{Log AL} = \tfrac{1}{4}\left(\frac{3\Sigma \log X}{n} + \log C\right). \quad (3)$$

The relative weights given to the training and test stimuli in the above formulae are approximate but not altogether arbitrary, since it is perhaps reasonable to assume that as one moves up the evolutionary tree, the relative in-

TABLE 1

DATA FROM KENDLER (15). AL CALCULATED FROM EQUATION (1)

(In this and the succeeding tables, where prediction is at variance with the observation, the calculated AL is printed in boldface.)

Condition I

N = 58. Training stimuli:* .063 (+), .011 (−)

Test Stimuli*	Per Cent Transposition			Calculated AL
	1st Trial	1st 5 Trials	2nd 5 Trials	
.37, .063	80%	80%	86%	.063
.37, 1.62	70%	72%	80%	.14
8.49, 1.62	60%	54%	80%	.31
40.46, 8.49	80%	72%	78%	.70
204.74, 41.22	67%	70%	92%	1.55

Condition II

N = 24. Training stimuli:* 8.49 (+), 40.46 (−)

1.62, 8.49	67%	77%	82%	8.29
.37, 1.62	33%	60%	79%	3.79
.063, .37	67%	60%	72%	1.68
.011, .063	50%	63%	69%	.70

* Brightness in apparent foot-candles.

TABLE 2

DATA FROM EHRENFREUND (3). AL CALCULATED FROM EQUATION (1)

Experiment I

N = 40. Training stimuli:* .22 (+), .91 (−)

Test Stimuli*	Per Cent Transposition		Calculated AL
	Trials 1–5	Trials 16–20	
.13, .57	86%	94%	.35
.09, .34	82%	74%	.28
.05, .22	48%	40%	.22
.035, .13	66%	54%	.17

Experiment II

N = 40. Training stimuli:* .13 (+), .035 (−)

.05, .22	84%	80%	.08
.09, .34	78%	86%	.11
.13, .57	28%	28%	.136
.22, .91	40%	50%	.17

* Diffuse reflection factor.

fluences of the presented situation and of previous learning will change in favor of the former. The training and test stimuli used in the experiments mentioned above, together with the percentage of relational responses observed, and the calculated test AL are given in Tables 1–4. The analysis is further condensed in Table 5 by considering those cases in which the predicted AL falls between and those in which it falls outside the test stimuli against the occasions on which more or less than 80% of the observed responses on the first five trials were in the direction of training.[2]

DISCUSSION

While the agreement between observation and prediction in the tables does

[2] Spence does not give data for the first five trials in his experiments. His results, which are for ten trials, have nevertheless been included in Table 5.

TABLE 3

DATA FROM SPENCE (25). AL (INDICATED IN PARENTHESES) CALCULATED FROM EQUATION (2)

Training Stimuli* and Subjects	Test Stimuli,* Percentage of Responses, and Calculated AL†		
160 (+), 100 (−)	256, 160	320, 200	409, 256
Pati	100% (182)	80% (201)	80% (250)
	100% (180)	100% (209)	60% (255)
Mona	50% (173)	40% (208)	80% (246)
	100% (183)	70% (212)	60% (255)
Pan	80% (173)	90% (208)	90% (246)
	100% (183)	100% (212)	100% (255)
256 (+), 409 (−)	160, 256		100, 160
Soda	50% (237)		40% (167)
	70% (219)		50% (161)
Bentia	40% (221)		30% (173)
	60% (227)		40% (166)
256 (+), 128 (−)	512, 256		128, 64
Cuba	90% (287)		100% (120)
Lia	100% (287)		100% (120)
256 (+), 160 (−)	409, 256		160, 100
Mimi	90% (277)		80% (153)
128 (+), 256 (−)	256, 512		64, 128
Jack	80% (273)		50% (114)
Nira	100% (273)		60% (114)
160 (+), 256 (−)	256, 409		100, 160
Bokar	80% (267)		50% (148)

* Area in square centimeters.

† The reader should consult Spence's article for details of the order in which the transposition tests were given. The AL's were calculated on the assumption that each test and each retraining series shifted the previous test or retraining AL to the same degree, irrespective of the number of trials given. It was also assumed that Cuba, Lia, and Mimi were tested first up and then down the size dimension, and that Jack, Nira, and Bokar were tested down and then up the scale.

TABLE 4

DATA FROM ALBERTS AND EHRENFREUND (1). AL CALCULATED FROM EQUATION (3) $N = 22$. Training stimuli:* 64 (+), 128 (−)

Test Stimuli*	Per Cent Transposition			Calculated AL
	1st Trial	1st 5 Trials	All 10 Trials	
64, 32	100%	100%	100%	53.8
32, 16	33%	73%	78%	31.8
8, 4	50%	53%	60%	11.3
4, 2	50%	50%	57%	6.7

* Area in square inches.

TABLE 5

SUMMARY OF TABLES 1–4. OBSERVED AGAINST PREDICTED FAILURE OR SUCCESS OF TRANSPOSITION IN FIRST FIVE TRIALS

No. of Tests in Which Per Cent of Transposition Responses Observed	Transposition Predicted	Failure of Transposition Predicted	Total
⩾80%	23	4	27
<80%	14	18	32
Total	37	22	59

χ^2 (Yates's correction) = 9.1; $df = 1$; $p = <.01$.

not merit any extravagant conclusions, it is perhaps sufficiently close to warrant brief discussion. With the introduction of further parameters the correspondence could, no doubt, be improved in some cases, though in others (e.g., Mona and Pan, who, in spite of receiving the transposition tests in identical order, managed nevertheless to give divergent results [25]) this might not be possible without a greater knowledge of the *S*'s previous history. Since we are not interested in transposition as such, but rather as a special case of equivalence behavior to which a particular kind of analysis seems appropriate, we shall not pursue the matter of precision further.

Next we shall consider the assumptions we have made in transferring Helson's model from its perceptual context to that of learning. Most exception will probably be taken to the assumption that discrimination learning is a two-stage process. While this is not the place to enter into the current controversy on this issue (see, for example, 2, 14), we may perhaps make a point to which recent writers have not given much attention, and which is directly relevant to the hypothesis advanced here. There is ample evidence, in certain restricted fields of human activity at least, that some form of behavior modification takes place in the absence of either reward or knowledge of results. Perhaps the clearest examples of this are to be drawn from the changes which repetition brings about in subjective scales (13, 27) in visual illusions (18, 19, 22), and in the skills studied by Gagné and his associates (4, 5). Whether we call this learning or not does not seem to be of much consequence. Inasmuch as psychophysical experiments are essentially concerned with discrimination behavior, and as they have produced evidence that predictable changes occur in this behavior which cannot be ascribed either to drive reduction or to sign-learning, it becomes not unreasonable to suppose that such changes continue to take place when the discrimination situation also involves the learning of some new set of responses. The assumption is necessary in the present instance to account for the continued increase in the number of transposition responses in the test trials, where every response is rewarded (1, 15, 16).[3] The other assumptions are additional to those of Helson only in the sense that they translate his postulates into a learning terminology; as such they do not require further discussion.

Finally, something should be said of the possible extensions of the approach advocated here. We shall concern ourselves first with deductions from the hypothesis. This has already been tailored to fit three established sets of facts: the occurrence of transposition, its diminution with the distance of test from training stimuli, and the increase of relational responses with repeated testing. One deduction has already been mentioned: that if the test AL coincides with the training AL, perfect transfer should result.[4] A second is to the effect that in cases where transposition is initially less than perfect, but improves with testing, there will be an inverse relation between the number of test trials

[3] This fact is not readily accounted for by Spence's (25) theory. On the other hand, granted the assumption of generalization gradients, Spence (26) accounts nicely for the difficulty of the intermediate size problem, which we are unable to do at present.

[4] Since this paper was written, D. H. Lawrence (*J. comp. physiol. Psychol.*, 1952, 45, 511–516) has reported data which are in agreement with this inference. His test stimuli were brightnesses of 31.8 and 25.9 apparent foot-candles. The AL's of his ATG1, GTG, and ATG2 groups, calculated from equation (1), are estimated for these test stimuli at 31.92, 30.7, and 30.37, respectively. Positive transfer is therefore predicted for the last two groups and negative transfer for the first.

carried out and the number of "correct" responses observed when one switches back to the original training pair. More complex effects, corresponding to the establishment of learning sets (8; 20, p. 129), will presumably result if this process is carried out too often.

Before the theory can be extended beyond such elementary, first-order deductions, however, it will probably be necessary to fill certain gaps in our knowledge. One of the more obvious is to define the limits of improvement which will occur with testing. A pinhead and a cartwheel do not lie on the same psychological dimension, and an examination of the relation between these discontinuities and the value of the anchoring point would not be without interest. Another problem is to determine, under a number of conditions, the relation between the amount of practice given on the training stimuli and the rate at which the AL shifts. This is of some importance if the present theory is to be extended to cover the intermediate size problem, which takes animals much longer to learn than does a conventional discrimination (26, 29).

At a more general level, it should be pointed out that the AL concept makes nonsense of any attempt to examine the basis of equivalence by the successive presentation of different stimuli under conditions where the sequential effect cannot be taken into account. If every stimulus alters the frame of reference against which the next is judged, it would seem that the difficulties confronting a qualitative analysis of this process are at present insuperable.

SUMMARY

1. It is argued that a psychophysical theory of stimulus equivalence avoids certain problems of sensory-sensory and sensory-motor integration which arise out of the application of S–R and Gestalt theories to this area.

2. It is shown that the main facts of transposition behavior, as a simple case of stimulus equivalence, can be derived from an extension of Helson's theory of adaptation level.

3. Some further problems suggested by this approach are briefly discussed.

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